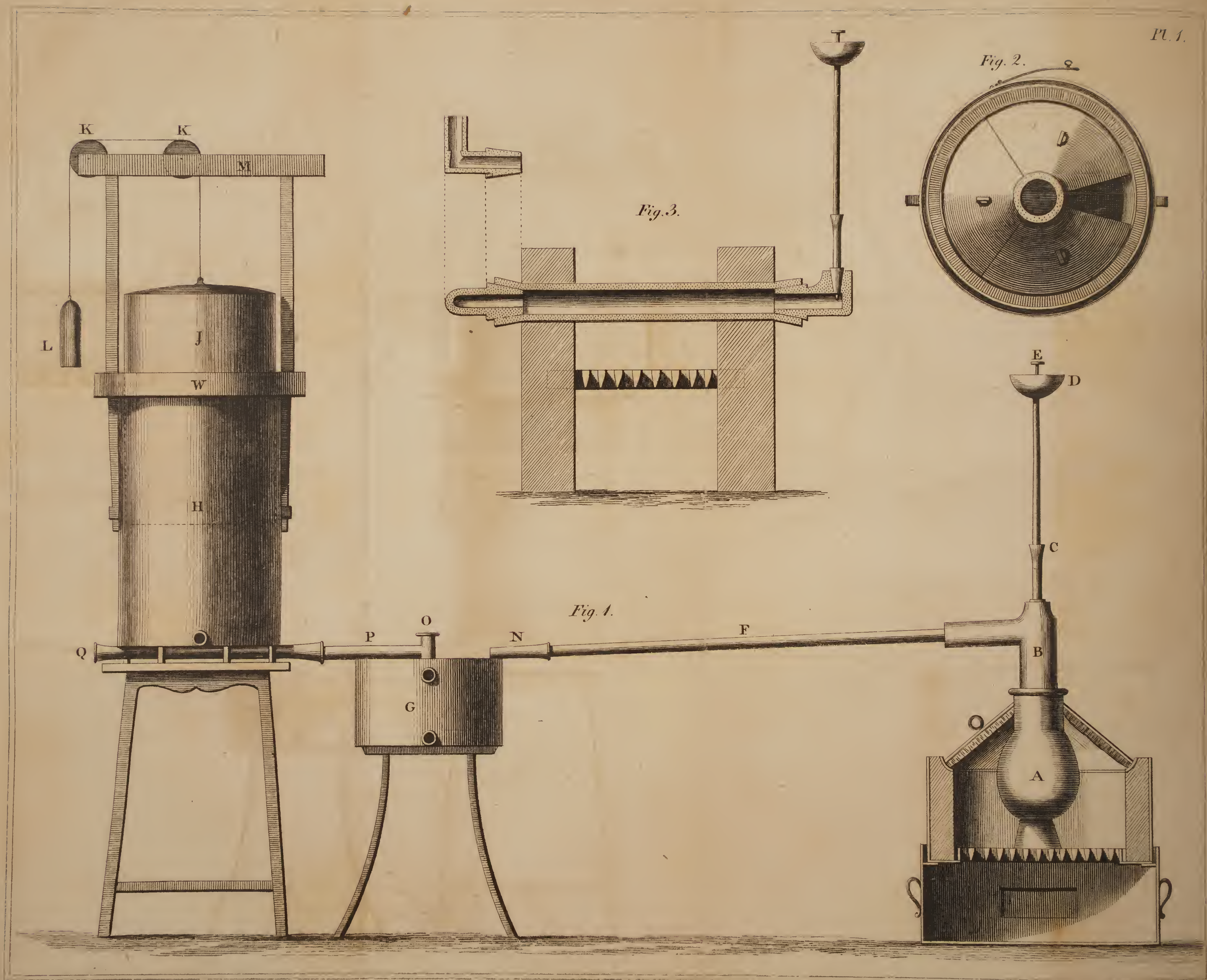
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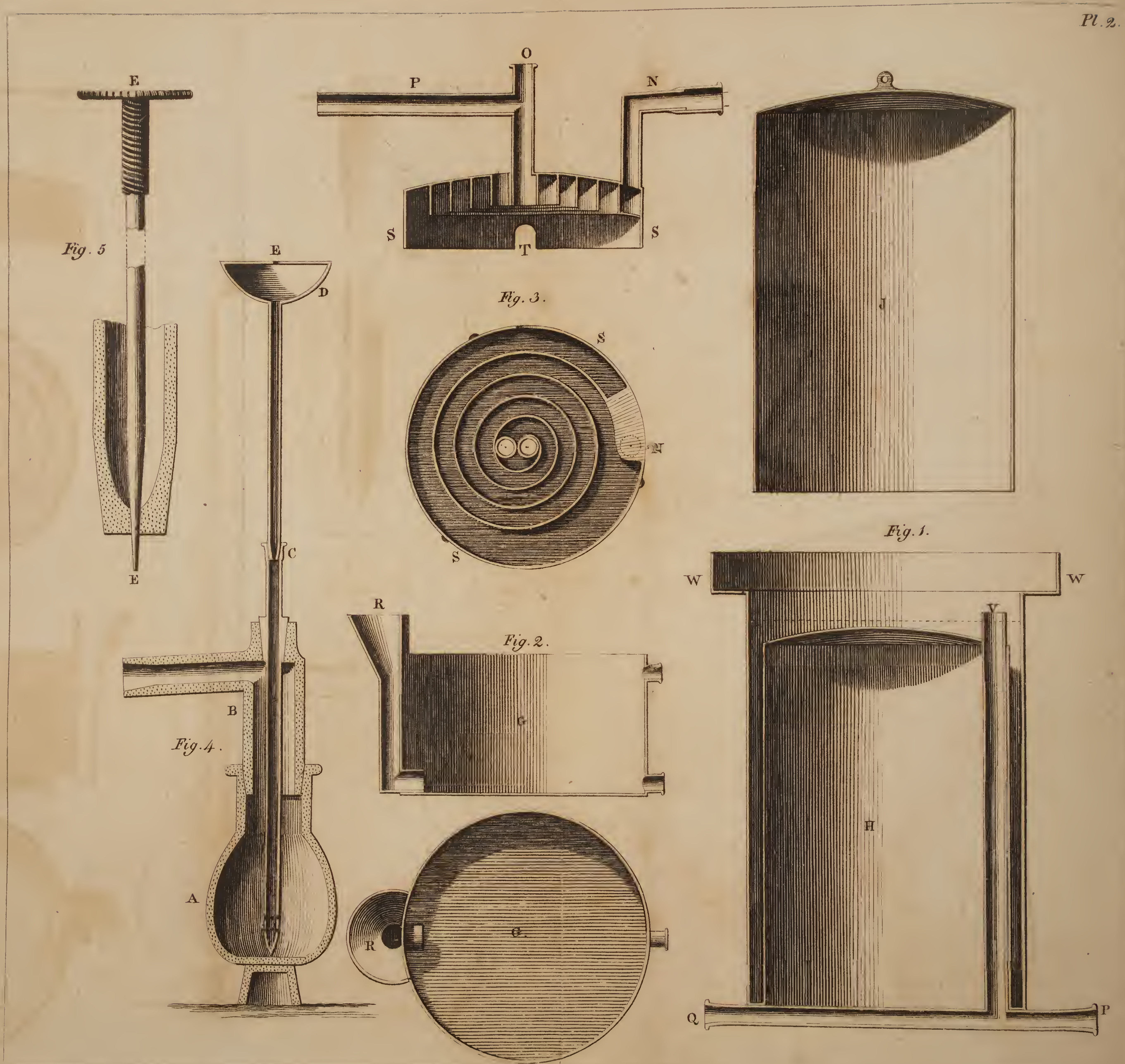
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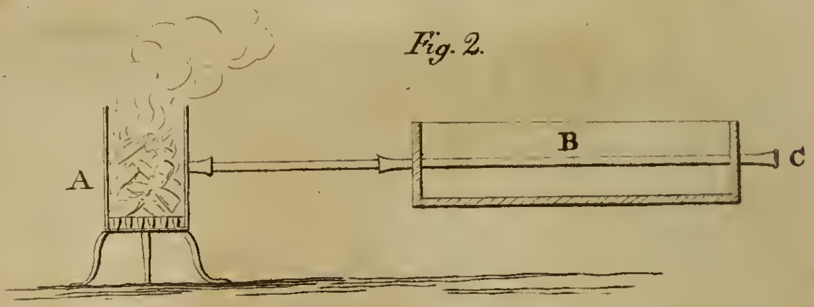

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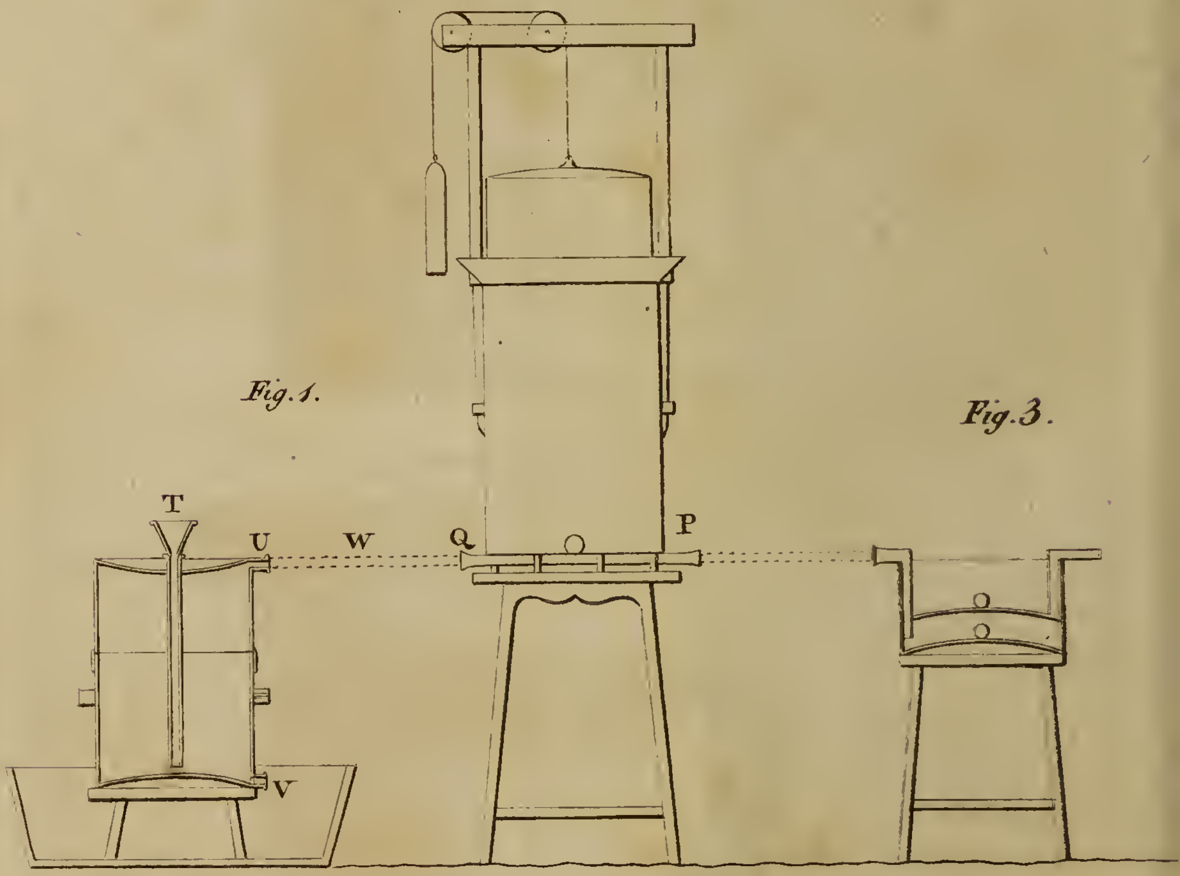

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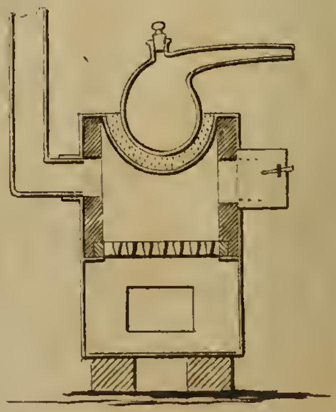

CONSIDERATIONS

ON THE

MEDICINAL USE

OF

FACTITIOUS AIRS,

AND

ON THE MANNER

OF

OBTAINING THEM IN LARGE QUANTITIES.

IN TWO PARTS.

Part I. by THOMAS BEDDOES, M. D.

Part II. by JAMES WATT, Esq.

BRISTOL:

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PRICE TWO SHILLINGS AND SIXPENCE.

AT *the close of the year* 1792, *three of my friends conceived that some good might arise from an experimental investigation of those physiological conjectures which I had lately published. They accordingly offered to bear a part of the expence attending the construction of a pneumatic apparatus, and the salary of a person to construct and superintend it. Without their co-operation, I should probably have attempted nothing of this kind. They agreed to risque with me the sacrifice of* TWO HUNDRED POUNDS *each. Now, as I firmly believe that the condition of humanity will be improved in consequence of the application of pneumatic chemistry to medicine, justice requires me to name the persons who shewed so liberal a spirit in promoting the attempt. They are* Mr. WILLIAM REYNOLDS, *and* Mr. JOSEPH REYNOLDS, of Ketley, *and* Mr. WILLIAM YONGE, *Surgeon of* Shefnal, Shropshire.— *What* Mr. WATT *has contributed to this pamphlet, shews that I have been further nobly assisted.*

Many medical practitioners have approved the design. Some, as I have been informed, go about decrying it as vehemently as philosophers of perhaps the same stamp, in a former age, decried the discovery of HARVEY. *I, however, make no pretensions to discovery, and have merely endeavoured to promote investigation in cases where either uniform failure, or frequent want of success, proves how much we need something better than we possess; and, to pronounce before trial, that this may not be in part supplied by elastic fluids—substances so active in Nature—may, without breach of charity, be construed as arguing the most arrogant presumption, the most sordid desire of gain, brutish ignorance, or personal spite.—As to hypotheses, I hold that their sole value consists in suggesting new experiments, and in procuring more accurate observations: assuredly, therefore, if I cannot rival them in their better part, I shall not imitate certain great men in the weakness they have discovered, by fond attachment to untenable opinions.*

Men of science, without any exception known to me, have expressed a wish that the enquiry should be prosecuted as extensively as possible, till we arrive at some certain conclusions. This also, as far as I can collect, is the sense of the Public. It has occurred to me, that some plan, like the following, would most speedily and certainly determine the medicinal virtue of factitious airs, and therefore most effectually put Society in possession of the benefit they are capable of affording, whatever that may happen to be.

PROPOSAL.

I FLATTER myſelf, that in a work entitled, *Obſervations on Conſumption, Fever, and other diſeaſes*, in my *Letter* to *Dr. Darwin*, and in a late collection of *Letters* from different correſpondents on the ſubject of Pneumatic Medicine, it is abundantly proved, that the application of factitious airs to the cure of diſeaſes, is both practicable and promiſing. This method of treatment has been very lately adopted abroad, and appears, as far as it has been tried, to have exceeded rather than diſappointed expectation. The following experiments on animals, and ſome clinical obſervations, of which an account will ſoon be given to the Public, muſt, I think, confirm the hopes entertained by many friends of humanity, concerning the medicinal efficacy of elaſtic fluids. Although, however, I might be allowed to ſuppoſe that enough has been done to encourage further enquiry, I am ſenſible that facts are wanting to eſtabliſh general concluſions. To what preciſe extent, therefore, the new mode of practice may be advantageous, remains to be decided by cautious experience.

This object, I conceive, may be much more effectually accompliſhed in two years, by means of a ſmall appropriated *Inſtitution*, than in twenty years of private practice: and perſons of high reſpectability, both belonging to the medical profeſſion, and others, have expreſſed their wiſhes that ſome attempt might be made to carry ſuch a deſign into execution. They are alſo of opinion, that an adequate ſubſcription may ſpeedily be raiſed, ſince nothing is more urgent than to reſtore health, and preſerve life.

Such an Inſtitution ſhould be conducted with a view to the attainment of two objects.—1. To aſcertain the effects of theſe powerful agents in various diſeaſes; and 2. To diſcover the beſt method of procuring and applying them.

The fidelity of medical narrations is of immenſe importance. But the publications of the fraudulent and the undiſcerning have almoſt deſtroyed all confidence in reports of ſucceſsful treatment. No means, therefore, of ſecuring authenticity, ſhould be neglected. The whole buſineſs ſhould be conducted in the moſt open manner poſſible, ſecrecy of

any

any ſort being manifeſtly incompatible with a deſign, calculated for the univerſal benefit of mankind. Hence, not only ſubſcribers, but others ſhould be admitted to enquire and inſpect at convenient times. It is ſcarcely neceſſary to add, that the greateſt care ſhould be taken to aſcertain the nature of each caſe, and to regiſter the changes produced by the airs, as well as every other particular relative to the patient. A dwelling-houſe, capable of receiving twelve patients, may, as it appears to me, be made fully to anſwer the purpoſe; ſince in many caſes the airs may be adminiſtered without keeping the patient conſtantly in the houſe. In two or three years, ſuch an eſtabliſhment ought to render itſelf uſeleſs, by ſo far ſimplifying methods and aſcertaining facts, that every practitioner of medicine, at leaſt, may both know how to procure and how to apply the different elaſtic fluids, ſuppoſing they ſhould be found ſerviceable in any ſpecies of diſeaſe.

The other articles of expence do not ſeem extremely formidable. They may be reduced to the following heads:

1. Houſe rent and furniture.—2. Air apparatus and materials.—3. Salary of a Medical Superintendant, anſwering to the Houſe Apothecary of hoſpitals, whoſe buſineſs ſhould be to direct the chemical proceſſes, and to adminiſter airs and medicines under the direction of the Phyſician.—4. A man ſervant, to aſſiſt the ſuperintendant.—5. Two female ſervants, one a nurſe.—6. Contingent expences of advertiſing, paper, printing.—7. Medicines.

It would further be deſirable, that the Superintending Committee ſhould be enabled to give premiums for the communication of ingenious methods of procuring, purifying, and adminiſtering airs. In this department, much remains to be done.

For the whole, three or four thouſand pounds would probably ſuffice; but the plan might be contracted or enlarged, according to the amount of the contributions. At all events, it ſhould be underſtood that no ſecond application would be made for ſubſcriptions.

To obviate miſapprehenſion, it may be proper to remark, that the propoſed inſtitution ought not to be confounded with ordinary charitable foundations, either with regard to its *object*, or to its *duration*. It is not for the ſake of relieving that diſtreſs which ariſes from poverty, but that which ariſes from the imperfect ſtate of medicine, that the preſent propoſal is ſubmitted to public conſideration. From this latter ſpecies of diſtreſs it is evident, that no degree of affluence can exempt any individual. Relief is only to be found in more powerful means of cure, or in a more ſkilful application of

the means, already in uſe. The exiſtence of dangerous and even incurable diſeaſes, furniſhes lamentable proof of the neceſſity of ſuch improvements in the moſt important of all the arts: with the great frequency of ſuch diſeaſes, the common occurrences of life allow no one to be unacquainted.

As the firſt ſtep neceſſary towards the execution of a deſign which depends upon public patronage, is to make application to the Public, I take the liberty, at the riſque of that diſgrace which ſometimes follows diſappointment, to propoſe

1. That perſons diſpoſed to contribute to a MEDICAL PNEUMATIC INSTITUTION, give in their names and ſubſcriptions to

Mr. Thomas Coutts and Co. Strand.

Eſdaile, Hamett, Eſdaile, and Co. Lombard-Street.

Meſſrs. Pybus, Call, Pybus, Grant and Hale, Old Bond-Street.

Meſſrs. Ranſom, Moreland, and Hammerſly, Pall-Mall.

Meſſrs. Smith, Payne, and Smiths, George-ſtreet, Manſion-Houſe.

Meſſrs. Staples, Newman, Anderſon, Staples, and Lynn, Cornhill.

2. That the ſubſcriptions be veſted in the names of Sir Benjamin Hamett, M. P. Alexander Anderſon, Eſq. and John Grant, Eſq. of Waltham Place, Bankers in London, who have obligingly undertaken to act as truſtees to the Inſtitution; and who will diſpoſe of the ſums ſubſcribed, as a Committee to be appointed by the ſubſcribers ſhall direct.

3. That the ſubſcriptions be advertiſed.

4. That before March 1, 1785, a general meeting of the ſubſcribers be called at ſome convenient place in London, in order to appoint a Committee, to fix upon the ſituation of the Inſtitution, to chooſe a Phyſician, &c. &c.

5. That the plan formed by the Committee be tranſmitted a month before its execution to each ſubſcriber for his ſuggeſtions.

It is, I believe, in the higheſt degree improbable that ſuch an eſtabliſhment ſhould be totally unproductive of benefit. But even in the worſt event, to have the merit of the project decided by a proper trial, will afford a ſort of melancholy ſatisfaction to perſons labouring under diſeaſes at preſent invariably fatal, and to their friends. For as it is generally known both that new means of relief have been propoſed, and that the inefficacy of theſe means has not been determined by experience, it is eaſy to imagine how diſtreſſing muſt be the feelings of both parties, eſpecially thoſe

of

of the defponding fufferers, when they find themfelves unable to procure a fupply of elaftic fluids at home, and when their circumftances will not allow them to feek the only chance of recovery abroad. Whereas, if they could be fatisfied that the means, which they defire in vain, have been tried and found inadequate, all regret on this account would ceafe.

Although I have always ftrictly confined myfelf to arguments in behalf of *a trial* of airs in medicine, *without giving the fmalleft affurance of fuccefs*, it may be thought that the Inftitution ought to be confided to a phyfician lefs prejudiced in favour of the project, than I can be fuppofed to be. In this decifion I fhall cheerfully acquiefce. If, however, a contrary opinion fhould prevail, my fervices fhall not be withheld from the Inftitution.

Should the prefent application be totally neglected, either as unworthy of regard, or becaufe defigns, capable of promoting the general welfare, may eafily fail to excite intereft, even when they do not provoke ridicule, I fhall ftill direct elaftic fluids in thofe difeafes, which continue the *reproach of medicine*, whenever I perceive the profpect of an happy iffue. In whatever cafes the practice proves ufelefs or difadvantageous, I fhall as earneftly diffuade from it as I before advifed the trial. For although it is confeffedly meritorious to explore the powers of nature, to mifreprefent them where health is concerned, appears to me a flagrant crime againft fociety.

MALL, CLIFTON, *Sept.* 30, 1794.

Whether this plan be executed or not, it is fatisfactory to reflect, that Pneumatic Medicine is now in fuch a train, that neither violent obloquy, nor artful infinuations, can hinder it from proceeding, and its value from being fooner or later determined. The practice will be much facilitated by the machinery defcribed in the following pages; and, in a few weeks, its efficacy, in fome formidable diforders, will be fhewn by the publication of well authenticated facts.

PART

PART I.

A

FAMILIAR EXPLANATION

OF THE

Principles on which Benefit may be expected from FACTITIOUS AIRS IN VARIOUS DISEASES.

I.—*Of the atmosphere.*

IT is proved, by satisfactory experiments, that the lower region of the atmosphere consists of two kinds of air, quite distinct in many properties. One is the kind called VITAL, DEPHLOGISTICATED, or OXYGENE AIR, and by a variety of names besides. The other has been named AZOTIC, PHLOGISTICATED, FOUL, or BAD AIR. Where the lower atmosphere is not altered by the breathing of animals, the burning of fuel, by exhalations from subterraneous chemical processes or putrefying substances, and such local causes, if you confine and examine an hundred cubic inches, you will find twenty-seven or twenty-eight to be oxygene, and the remaining seventy-two or seventy-three azotic air. The manner in which air may be imprisoned and examined, is described in the writings of Dr. Priestley, Mr. Scheele, Mr. Cavendish, and Mr. Lavoisier. These authors explain much of the nature of oxygene and azotic air. A candle burns in a vessel full of oxygene air with dazzling brilliancy, and is consumed with great rapidity. This air turns various substances sour, when it unites with them, as beer, milk. It changes black blood from a vein

to

tō a bright, florid, ruddy colour. You may ſee this difference of colours, by breaking a clot of blood that has ſtood a little time in the air; the ſurface will be ruddy, the inſide dark coloured. The reaſon is, becauſe the ſurface of the blood draws to itſelf ſome oxygene from the atmoſphere. When black blood is put into azotic air, it does not become ruddy. Azotic air extinguiſhes flame, does not burn when mixed, or in contact, with common air, and is not abſorbed by lime-water.

Near the earth, theſe two airs are found mixed with ſurpriſing exactneſs. Take a cubic foot from ten different places, and you will find that a little more than a quarter of each is a little *fixed* or *carbonic acid* oxygene; the reſt azotic; air is often found, as one part in an hundred, though no fires burn, or animals breathe near. The nice balance of attraction between the two conſtituent parts of the atmoſphere, deſerves notice. Theſe two ſubſtances, when cloſely united, form nitrous acid: If, therefore, they had a ſtronger attraction, or were not, by ſome circumſtance, prevented from uniting, all the oxygene, and part of the azotic, would be changed into this highly corroſive acid. Again, azotic is lighter than oxygene air; if, therefore, they had not ſome attraction, they might ſeparate, and any animals, that ſhould be immerſed in an atmoſphere of azotic air, would almoſt inſtantly expire: The undiluted oxygene remaining below, would, as we ſhall preſently ſee, occaſion violent diſeaſes in quadrupeds with warm blood and in man, as well as in many other animals.

II.—*Of the breathing of man and animals with warm blood.*

If you fix a pipe to a bladder full of air, and, holding your noſtrils, breathe this air for ſome time, your diſtreſſed feelings will inform you that it is no longer fit for breathing. If you transfer this breathed air into an inverted glaſs jar full of water, and turn up the jar ſo as to keep in the air, and admit none from the atmoſphere, you will find that it extinguiſhes a candle, and deſtroys the life of a ſmall animal, dipped into it. If you procure another quantity of this air, and add to it a little

more

more than one fourth of oxygene air, a candle will burn in it juſt as in the atmoſphere; and you may breathe it as long as ſo much freſh air, though it is not exactly the ſame; for it contains, after being breathed, ſome fixed or carbonic acid air, either thrown out from the blood, or formed in the lungs. Theſe experiments indicate, that breathing renders common air unfit for ſupporting life or flame, by depriving it of oxygene. Various other experiments, by the philoſophers above-mentioned, ſhew further that this is the caſe. The blood, before it paſſes through the lungs, is dark; after paſſing, it is florid; dark blood in a bladder, expoſed to the atmoſphere, becomes florid ſuperficially; and in breathing, the blood and air are only ſeparated by membranes not unlike a bladder. Dark blood, introduced into veſſels containing oxygene or common air, leſſens its quantity, as it becomes ruddy. Hence it appears, that the blood drinks up a portion of the oxygene air received into the lungs; and from various conſiderations, this appears to be uſed in the contraction of the muſcles, in ſeveral fluids, ſecreted from the blood; for the blood, after traverſing the body, comes back to the lungs dark, or without the oxygene, which it received in paſſing through them. It has been calculated, that an healthy man requires nearly five cubic feet of air, or $1\frac{1}{2}$ cubic feet nearly of oxygene air, every hour.

So much is premiſed to render the following experiments and ſpeculations intelligible to ſome readers.—They will find more in Dr. Goodwin's *connection of life with reſpiration*, Mr. Coleman's *diſſertation on ſuſpended reſpiration*, Dr. Menzies' *Tentamen de reſpiratione (Annales de Chimie*, 1791, p. 211), in my three publications on the propriety of employing elaſtic fluids in various diſorders, and the chemical authors already quoted.

It appears that the ſkin imbibes and exhales air. It will imbibe various kinds; but, as it is found in equal times to take in three or four times as much oxygene air as any other, it probably ſelects oxygene alone from the atmoſphere. Some philoſophers ſuppoſe the human ſpecies to have exiſted in a monkey ſtate; would the hair then ſo much prevent the cutaneous abſorption of oxygene as the cloaths at preſent?

III.

III.—*Though the proportion of oxygene in the atmoſphere may be beſt adapted to the average ſtate of health, may the proportion not be ſmaller than is beneficial in ſome diſorders, and larger than in others?*

Conſiderate perſons will, I conceive, reply that this is probable. I have made many experiments on animals, to illuſtrate the effect of atmoſpheres of various conſtitution. I ſhould have made more, had I not been abſent from England, or travelling for the greater part of the laſt eight months. No inveſtigation of greater importance to humanity, or extent, can be imagined. This is only a rude beginning. Others will aſſiſt in continuing the enquiry.

IV.—*The effect of breathing oxygene air undiluted.*

Dr. Prieſtley and Mr. Lavoiſier found animals either to die, or to become exceedingly ill in ſuch air, while it continues more oxygenated than the atmoſphere, and will ſupport the life of other animals. It is not then defect, but exceſs of oxygene, that is pernicious here. The heart and arteries pulſate more quickly and forcibly; the eyes grow red and ſeem to protrude; the heat of the body is ſaid conſiderably to increaſe (a), ſweat to break out over the whole body, and fatal mortification of the lungs to come on. Theſe appearances denote violent inflammation: Animals have always appeared to me to ſuffer extremely ſoon after immerſion in unmixed oxygene air. To my own lungs, it feels like ardent ſpirits applied to the palate; and I have often thought I could not ſurvive the inſpiration of oxygene air from mangane by heat many minutes. The exiſtence of inflammation is fully eſtabliſhed by diſſection, as others have found, and as appeared from the following experiment. A large kitten was kept ſeventeen hours in a veſſel containing ſeveral cubic feet of air from manganeſe, of which about eighty parts in an hundred were oxygene. This, and another kitten of nearly the ſame ſize, which had lived as uſual, were then diſſected in my preſence, by Mr. Guillemard, of St. John's College, Oxford, who immediately made the following minute of the appearances:

(a) Girtanner Antiphlogiſtiſche Chemie, p. 263.

ances:—" The lungs were of a florid red colour in the
" oxygenated kitten (A); in the other (B), they were
" pale; the difference was very ſtriking, both in the in-
" flated and uninflated ſtate; the edge of one lobe in A
" was marked with livid ſpots (as in mortification). The
" pleura was likewiſe evidently inflamed. The heart in
" A was of a florid red colour. The liver, kidney,
" ſpleen, and blood veſſels of the meſentery and urinary
" bladder, were of a brightiſh red colour. In B, the
" heart was of a deepiſh colour. The liver, ſpleen,
" kidneys, and blood veſſels in general, were of a bluiſh
" or purple colour. Both kittens had been ſucceſſively
" killed by immerſion under water. Upon opening into
" the head, there was no appearance of inflammation.—
" The blood veſſels had rather a florid colour; but there
" was no ſign of extravaſation, or more than the uſual
" quantity of blood. In B, on raiſing the ſkull, there
" appeared a quantity of blood between the bones and
" the membranes of the brain, of which the blood veſſels
" were turgid with dark-coloured blood.

" In A, the heart readily obeyed the ſtimulus of
" pricking: The ſpontaneous contractions of the right
" auricle and ventricle were frequent; they continued
" with little diminution of frequency and force for above
" half an hour. In about an hour, they had wholly
" ceaſed.

" In B, the irritability of the heart was at firſt equi-
" vocal. On opening the pericardium half an hour after
" the ſternum had been removed, the motions of the
" heart became very viſible; they continued more than
" an hour after the firſt expoſure of the contents of the
" thorax."

The univerſally diffuſed florid colour in A was particularly ſtriking: So was the dulneſs of one heart at firſt, and the vivacity of the other: Of the latter, I believe the ſpontaneous pulſations in all were many times more frequent and forcible; though this circumſtance deſerves more particular examination than we beſtowed upon it. The kitten (A) had eaten ſome time after being put into the reſervoir, as appeared from the ſtate of ſome food introduced at the ſame time. The air ſeemed to have ſuffered little diminution either in quantity or quality:

The reafon will appear from a fubfequent experiment. On cutting the wind-pipe of A to blow up the lungs, a good deal of vifcid mucus flowed out. This was occafioned by ftrong action continued for fome time, and was not feen in B.

V.—*Experiments with air, containing fomewhat more oxygene than the atmofphere.*

In my letter to Dr. Darwin, I conjectured " that " Divers would be able to continue longer under water, " if before immerfion they were to breathe air of an " higher than the ordinary ftandard" (p. 13). I made feveral experiments to determine whether this fuppofition was juft; in each two animals of the fame litter were employed; and as feveral fpectators were fometimes prefent, they were defired to fix upon the weakeft for oxygenation. The following report I literally tranfcribe from my journal, as it was fettled and fubfcribed by the fpectators: " Auguft 20th, 1793. Kitten C was placed " in a mixture of nearly two-thirds oxygene air from " manganefe, and one-third atmofpheric air; it was " kept twenty minutes in the veffel, which was from " time to time fupplied with oxygene air, fo as to keep " the air better than atmofpheric air, which was known " by dipping a candle into it, and obferving that it " burned with a brighter flame. At the expiration of " the twenty minutes, C and D, which had breathed " atmofpheric air, were immerfed in water till perfect " afphyxia came on. At the inftant they were taken " out, there appeared in both a motion of the lower jaw; " C began fenfibly to recover, while D lay as dead: In " a minute and half, C rofe, and began to walk about " the room, ftaggering at firft, D being ftill motionlefs " or nearly fo; in this ftate it continued for fifteen mi- " nutes, when, for the firft time, it raifed itfelf, and im- " mediately afterwards fell on its fide.

" Christopher Machell.
" Richard Lovell Edgeworth.
" J. Guillemard.
" Jas. Sadler.
" Thomas Beddoes.

" Kitten D died the next day."

Of the other fimilar experiments, it is fufficient to obferve, that the refult was always in fome degree the fame; fometimes the unoxygenated animal failed to recover; it was generally noticed that the oxygenated fhewed figns of life under water the longeft; and fometimes that it ftruggled as much as ever after its oxygenated fellow had ceafed to move. Thus, in an experiment, September 28, a whelp, which had refpired atmofpheric mixed with one-third of oxygene air for thirty-four minutes, is regiftered to have been as much alive as before immerfion under water, another puppy of the fame litter unprepared, and immerfed at the fame time, having become motionlefs. Thefe facts illuftrate the query concerning divers. To obviate any miftake from difference of conftitution, the experiment was fometimes repeated upon the fame pair of animals, one being oxygenated one day, and the other the next, or the day following. The water in which they were drowned, was fometimes heated to the temperature of the body.

But as unequal quantities of liquid have been found to get down the wind-pipe of drowning animals, it feemed proper to repeat the experiment in another manner.— Accordingly, of two greyhound puppies of the fame litter, ten days old, E the weaker was kept an hour and fifty minutes in a mixture of two thirds of atmofpheric air, and one-third of oxygene air from heated mangane fe. F was left as ufual: Both were then immerfed in hydrogene air. F foon appeared much agitated, and expreffed much uneafinefs. E moved very little, and foon placed itfelf in the couchant pofture, with the head between the fore-legs and the muzzle refting on the bottom of the veffel. In five minutes, F was lying on its fide, now and then breathing, which it did lefs and lefs frequently and more feebly. In ten minutes, this effort was fcarce perceptible: In two minutes more, it was not once repeated. For the laft fix out of the twelve minutes, E was fo perfectly ftill, that we were difpofed to believe it dead; and a perfon prefent faid, "this experiment will turn out ill for oxygene." During thefe laft fix minutes, E had not infpired at all; and from the firft, the refpiration was very infrequent.

At the end of the twelve minutes, both puppies were taken out of the hydrogene air; E immediately cried

and

and ſtruggled, F being quite motionleſs. They were laid before a fire; E cried, moved, and ſoon walked as uſual; F ſeeming quite dead. In ſixteen minutes, a ſtream of oxygene air was blown into F's mouth, but no ſign of life appeared. The animal was afterwards opened; upon irritating the pericardium with a pointed knife, ſo as to preſs upon the heart, no movement followed; the pericardium being removed, the heart began to contract ſpontaneouſly; a ſtream of oxygene air being directed upon the heart, its action became more ſtrong and frequent; the number of ſtrokes was about ſeventy in a minute. The colour of the heart (probably from the filling of its own blood veſſels) changed from pale to red. The difference of colour in the tongues of theſe puppies was ſtriking even by candle light, after the experiment, that of E being much more ruddy. The following variation of the experiment ſeems worth tranſcribing from the journal: Of two puppies of the ſame litter, the weaker G was kept in atmoſpheric air mixed with one-third oxygene, and H for an equal time in atmoſpheric air with one-third hydrogene. Both were plunged into tepid water. H became motionleſs, while G moved with force, cried on being taken out, and ſeemed little affected.

The effect of oxygene air was very ſtriking in recovering H. It began to move, and reſpire the moment it was put into a veſſel containing this air.

It was ſometimes obſerved, that the movements of very young puppies under water, did not entirely ceaſe in leſs than fifteen minutes.

VI.—*Neceſſity of oxygene air to muſcular exertion.*

The blood in the veins is dark; in the arteries it is bright. When the reſpiration is ſtraitened, the arterial blood becomes darker; when acceſs of oxygene air is prevented, all the blood becomes dark. In drowned and ſtrangled perſons, the face, lips, the ſkin under the nails, and ſome other parts, are of a violet or dark blue colour. Here the blood can receive no oxygene.—There are a number of caſes on record, where, from bad conformation of the heart and adjacent great blood veſſels, part of the blood only paſſed through the lungs; the reſt paſſed in the dark diſoxygenated ſtate in which it returns from

from the veins, into the arteries again. Such perſons are always blue or livid. They are extremely feeble; in walking, are ſometimes obliged to ſtop every third ſtep, and cannot make any exertion of the muſcles without inſtant panting and wearineſs. They commonly die ſuddenly; you will find an account of ſuch individuals in *the Commentaries of the Inſtitution at Bologna.* Vol. 6, p. 64. *Philoſoph. Tranſactions,* vol. 55, p. 72. *Medical Obſervations and Enquiries, vol.* 6. in my *Medical Obſerv.* p. 62. *Abernethy's Surgical Eſſays, part* 2.— Perſons ill of ſea-ſcurvy, often drop down dead in making a ſudden effort, and from ſurprize. There is reaſon to believe, that either living in confined air, or on ſalted food, occaſions a deficiency of oxygene in the fluids and ſolids.

Hence, if a perſon were to keep quite ſtill, a given quantity of air ſhould ſerve him to breathe longer than if he exerted himſelf. Thus ſhould any perſons find themſelves again in the ſituation of Mr. Holwell and his fellow-ſufferers in the Black-hole priſon at Calcutta, their beſt chance of ſurviving would probably be to forbear vehement ſtruggles. The fever of the ſurvivors appears to have been occaſioned by the great ſtimulating power of freſh air, and of the ſenſations their eſcape muſt have occaſioned.

The following experiments render probable the expenditure of oxygene in muſcular exertion. Of two half-grown kittens of the ſame litter, one was teazed to make efforts for half an hour, and then put into an air-tight veſſel, in which it lived 48 minutes; the other lived 56 m. in the ſame veſſel; it would require more ſuch cruel experiments to decide whether ſpeedier death here aroſe from previous conſumption of oxygene by ſtrong muſcular action, and the ſubſequent neceſſity of a ſupply. It ſhould be obſerved, that the firſt animal was not reſpiring more deeply than the ſecond, at the time they were incloſed.

The following fact is remarkable, and countenances, but does not rigorouſly prove, the hypotheſis. A grown cat was incloſed in an air-tight glaſs veſſel. She immediately became furious to a degree beyond what I have obſerved in any animal under experiment. The violent

agitation continued for 20 minutes. In 5 minutes more—25 minutes in all—ſhe appeared dead; ſhe was left in the veſſel two minutes longer, and proved to be quite dead. A lighted candle was immediately extinguiſhed on being introduced into the veſſel.

Into the ſame veſſel another cat of the ſame ſize and age nearly, to which a ſmall glaſs of white wine had been given half an hour before, was introduced. This cat ſate almoſt perfectly ſtill during the whole experiment. It lived 47 minutes, or nearly twice as long as the other.

In order to vary the experiment, half a glaſs of ſherry was given to a kitten nearly grown. It was *immediately* put into the ſame receiver; and ſet to *ſtruggle very violently*. It ſoon appeared to reſpire with difficulty. In 15 minutes the reſpirations were 98 or 100 in a minute. It did not reſpire after the 34th minute, and in 2 minutes more was taken out inſenſible.

A fellow kitten, no way prepared, was placed in the ſame receiver, and remained *very tranquil* for above a quarter of an hour; its reſpiration was never ſo frequent as that of the former; and it raiſed its head and breathed at the end of 41 minutes.

We have then

	Minutes.	Minutes.
An harraſſed kitten living	48	Difference 8.
Its fellow, not previouſly harraſſed,	56	
A grown cat not prepared, but furiouſly agitated, - - -	25	Difference 22.
Another perfectly tranquil, having drank wine, - - -	47	
A large kitten immediately after wine, and violent, - -	34	Difference 7.
Its fellow tranquil without wine,	41	

In theſe ſix experiments the ſame veſſel, that is, the ſame quantity or air, was uſed. It may be ſaid, by a perſon unuſed to accuracy of terms, "no wonder the moſt exhauſted animals ſhould periſh ſooneſt." By conſidering a moment, he will perceive, that it is deſireable to know preciſely in what this exhauſtion conſiſts. I formerly conjectured that oxygene is conſumed faſter by an animal under the firſt operation of wine or other ſuch ſtimulants; and Dr. Withering afterwards adduced the

experience

experience of Mr. Spalding in confirmation of this conjecture. It is not so easy to make the experiment upon animals, the efforts of some under confinement being so much more violent than of others. The last experiment was made with a view to this question, but the two preceding incline me to refer speedier death in this instance to the violent struggles, rather than to the wine.

VII.—*Another comparative experiment with an Animal charged with oxygene.*

Of two half-grown rabbits (K and L) of the same brood, colour, size, and apparent strength, K was put into a large reservoir containing atmospheric air with a little oxygene. After some hours it was taken out, and placed for an hour in a mixture of nearly equal parts of oxygene and atmospheric air. It did not seem to suffer in its respiration ; K and L, which latter had remained at large in the same apartment, were then inclosed in a vessel, and placed in a freezing mixture. In 20 minutes some of the cold brine was poured upon the bottom of the vessel in which the rabbits were : in 30 minutes L seemed affected, in 45 was scarce alive, and in 55 was quite lifeless, and frozen stiff. K seemed sufficiently lively, only its feet were frozen stiff. They were dipped in cold water, and the animal recovered perfectly. I observed many convulsions and much tremor of the limbs during recovery. It was between 8 and 9 o'clock in the evening when the rabbits were taken out of the vessel. K, by 12, had recovered the use of its forelegs, and being left not far from a dying fire within the fender, was found in the morning running about the room, when it eat cabbage leaves freely. It was kept alive for a week, when the legs appeared diseased from too quick application of heat at first.

The experiment being repeated without admitting liquor into the receiver, the result was similar. Would opium and wine enable an animal to resist the freezing mixture, as oxygene does ?

VIII.—*Experiments with oxygene and other airs, largely distributed through the cellular ſubſtance.*

Dr. Maxwell, aſſiſted by Dr. Goodwyn and ſome other friends of accuracy and genius, forced different airs under the ſkin of animals, whence every perſon in any degree acquainted with anatomy, knows they would inſinuate themſelves far and wide through the body, in conſequence of the free communication between different portions of the cellular ſubſtance.—I. 4½ pints of *atmoſpherical air* were forced under the ſkin of a bitch, weighing 20lb.; the inciſion was cloſed by a ſuture: the animal appeared uneaſy and indiſpoſed for 36 hours; the puffing did not begin to ſubſide before the 9th day; on the 20th, no air was left except a little about the lower part of the belly.—II. 3 pints of air, in which a light had burned out, were forced under the ſkin of a dog weighing 13lb. For ſome hours the animal appeared ſtupid. The emphyſema or puffing ſeemed to decreaſe during the 3d day; on the 16th convulſions came on and frequently returned; on the 20th the dog died, much debilitated. In three other experiments nearly the ſame phænomena were obſerved.—III. 4 pints of oxygene air were infuſed in the ſame manner into another dog; ſlight uneaſineſs was obſerved for the firſt hour, and afterwards the animal appeared exceedingly lively (*maxima alacritas*). Next day the emphyſema began to leſſen; by the 10th all the air was abſorbed. In another dog of 19lb. 3½ pints of this air diſappeared in 8 days; in a third of 21lb. 3 pints in 8 days; in a 4th of 20lb. 3 pints nearly in 7 days. The 2d and 3d were affected as the firſt dog; the 4th was in no way affected.—IV. Carbonic acid air was infuſed into ſeveral dogs and rabbits. A large quantity (as much as 2 pints in a dog of 17lb) diſappeared during the operation; the reſt was gradually abſorbed in 4—14 days. No inconvenience followed, except in one caſe where a pint of air infuſed into a rabbit 3 months old, occaſioned uneaſineſs from diſtention; but even here the animal eat with a good appetite in half an hour. The inſtantaneous diſappearance of ſo much air in theſe experiments, was probably owing to

to its combination with the moiſture in the cellular ſubſtance.—Inflammable air (from metallic ſolutions, I ſuppoſe) occaſioned heavineſs and ſhivering in two dogs; 3 pints in one, $2\frac{1}{2}$ in the other. Some detumeſcence was obſerved on the 4th day in both; in 13 days the air was all gone in the 1ſt, and in the 2d in 9 days.—VI. $2\frac{1}{2}$ pints of nitrous air were infuſed into a dog of 28lb. It howled as if in exquiſite pain: in 15 minutes it ſtaggered as if drunk; then convulſions came on, and vomiting with involuntary excretions. In 30 minutes it lay enfeebled on the ground, making deep and laborious inſpirations, in $54\frac{1}{2}$ it died, the convulſions continuing to the laſt.—The heart had all its cavities full, and was quite inirritable. The lungs were of a pale ſaffron colour, and ſhewed no veſtige of red blood. Brain in a natural ſtate. In another experiment $1\frac{1}{2}$ pint of nitrous air produced the ſame effects, and death in 45 minutes. In neither caſe were the external muſcles inirritable. Rabbits died juſt as theſe dogs, and the ſmell of nitrous acid was perceived when the lungs were inflated and left to collapſe. In this theſis (Edinburgh 1787) Dr. Maxwell relates other experiments, in which airs were thrown into the blood-veſſels. By one (p. 22) he ſhews that elaſtic fluids do not prove fatal till they get into the cavities of the heart. But as theſe latter experiments ſuggeſt no concluſion concerning the medicinal power of elaſtic fluids, I need not conſider them at preſent. Mr. Achard of Berlin, was the firſt who publiſhed experiments with different airs injected into the cellular membrane. But Mr. Achard is a writer whom one can ſeldom quote with confidence.

IX.—*Experiments with hydrogene and other mephitic airs.*

Dr. Prieſtley, (*Exp. on Air*, *N. Ed.* I. 229,) ſays, "Inflammable air kills animals as ſuddenly as fixed air, "and as far as can be perceived, in the ſame manner, "throwing them into convulſions, and thereby occa-"ſioning preſent death." Dr. Prieſtley does not ſay how he aſcertained the former part of this aſſertion, and I apprehend, it will be found erroneous, if it regard inflammable air from zinc or malleable iron with ſteam. Mr. Scheele could make 20 inſpirations without inconvenience; and I have ſeen ſeveral perſons breathe

breathe ſtill oftener from a tube through which a current of this air ſet, their noſtrils not being cloſed *(Letter to Dr. Darwin, p. 44)*. Hence I concluded that this bland air might with impunity be breathed unmixed, longer than any other mephitic air, except perhaps azotic. Dr. Macdonnel of Belfaſt, whoſe abilities and ſkill in phyſiological reſearches muſt be well remembered by all who ſtudied medicine at Edinburgh 8 or 9 years ago, confirms me in this opinion. " I have tried, (he informs me in a letter dated Auguſt 13, 1794), " hydrogen air " in five pulmonary caſes, in two of which it had a very " ſudden and a very favourable influence. In one of " the others the meaſles ſupervened upon phthiſis, and " ſeemed to decreaſe the firſt diſeaſe.—My patients " ſometimes reſpired hydrogen air for a minute and half " at a time; the more frequently they repeated the ex-" periment, the more eaſy did it become; but after 15 " or 20 inſpirations I always obſerved the face to grow " dark and livid. I am aſtoniſhed at the length of time " which man can breathe, and animals live in, hydrogen " air."

Dr. Gilby of Birmingham noted the following appearances, and immediately afterwards drew out this minute.

" *Hydrogene Air.*"

" A mouſe immerſed in hydrogene air—from water " and heated malleable iron—continued 30 ſeconds with-" out ſhewing any mark of diſtreſs; reſpiration then " became laborious; one minute 33 ſeconds from the " time of immerſion it inſpired; but it moved no more, " and when taken out, proved to be quite dead.

" *Fixed, or Carbonic acid Air.*"

" Another mouſe, immerſed in this air, was inſtantly " affected; and in 15 ſeconds was completely dead." A young wood pigeon, in hydrogene air, ceaſed to gape and move in 2 minutes 35 ſeconds. For 10 or 15 ſeconds it did not appear incommoded. Its fellow, in carbonic acid air, ceaſed to gape and move in 43 ſeconds. It ſhewed diſtreſs inſtantly on immerſion.

Young

Young animals do not drown ſo ſoon as old.—Imagining, therefore, that young animals would afford a more ſenſible ſcale on which to meaſure the power of different mephitic airs, to extinguiſh life, I made the following experiment. A puppy, four days old, was put into a veſſel of hydrogene air from heated iron and water. It ceaſed to breathe and move twenty-two minutes afterwards.—Another puppy, of the ſame litter, was put into carbonic acid gas, it ceaſed to breathe and move in one minute and an half.—Comparative experiments of this kind require repetition; two apparently ſimilar animals may be tenacious of life in different degrees, from cauſes not yet diſcovered; moreover, if immediately before immerſion, one ſhould have inſpired, and the other expired, this might occaſion a wrong inference. By keeping animals, ſeemingly equal, in different unreſpirable airs, till all appearances of life in one or the other had ceaſed; then taking the ſurvivor out, ſuffering it to recover, and after ſome days drowning it again in that air in which its fellow had periſhed before, I hoped to determine this queſtion certainly for the ſubjects of experiment, and by analogy for all animals of the ſame claſs.

Accordingly, three rabbits of the ſame litter, ſeven weeks old, much about half grown, and weighing one pound and an half each were ſucceſſively immerſed in three different kinds of air. Dr. Gilby being preſent at this experiment alſo, noted the appearances at the moment they occurred.

Experiment I.—Rabbit X.

" *In hydrogene from water and heated malleable iron.*

	Minutes,	Seconds, after immerſion.	
" In	1	20	Moved about, in appearance little diſtreſſed.
	1	50	Began to breathe ſhort.
	2	0	Viſibly diſtreſſed.
	4	15	Much agitated.
	7	0	Taken out, breathing very ſhort and thick.
" In leſs than	17	0	Completely recovered.
" In	40	0	(Which was ſoon as food was offered,) began to eat.

" Exp.

" Experiment II.—Rabbit P.

" *In hydrocarbonate air from hot charcoal and water, twice passed through water.*

	Minutes.	Seconds.	
" In	0	25	Breathed ſhort, diſtreſſed.
	0	35	Violently agitated, continued ſo 15 ſeconds; inſpired at long intervals for ſome ſeconds: ſcarce alive.
" After	1	30	No inſpiration or movement ſeen.
" In	4	0	Taken out for dead—did not recover.

" Experiment III.—Rabbit Q.

" *In carbonic acid air, from heated chalk.*"

	Minutes,	Seconds, after immerſion.	
" In	0	20	Strongly convulſed.
	0	35	Gaſped at intervals.
	0	49	Has continued gaſping.
	1	0	Nearly dead.
	1	15	Quite dead.
	2	0	Taken out, perfectly lifeleſs, did not recover.

Experiment IV.—Rabbit R.

At the time of making theſe experiments I had not pure azotic air at command, and had neglected to uſe it when I had; the following obſervation makes it highly probable, that this air is not more ſuddenly deleterious than hydrogene. A candle having burned out in a veſſel full of atmoſpheric air in contact with lime water, a very ſmall kitten (about 14 days old) was put into the ſame portion of air; after the death of this kitten, which did not happen in leſs than 3 hours, the fellow of the three preceding rabbits was introduced; the following were the appearances:

Minutes.

Minutes.

1	Breath ſhort—turns round.
3	In no great diſtreſs, breath ſhort.
5	The ſame.
7	Breath ſhorter.
10	Reſpiration apparently more laborious.
12	Taken out—very ſoon recovered—a candle plunged into the veſſel was immediately extinguiſhed.

EXPERIMENT V.—RABBIT X *again.*

In hydrogene air, at the interval of ſeveral days.

	Min.	*Sec.*	
	——	——	At firſt very tranquil.
In	2	0	Snuffs for air round the ſides of the veſſel.
	4	0	Reclined almoſt on its ſide.
	5	30	Breathes thick—very weak.
	6	10	Taken out, breathing thick,
	7	10	Could ſit.
	8	0	Could move, though ſtill weak.
	9	30	As uſual.

EXPERIMENT VI.—RABBIT X *a third time.*

Recent hydrocarbonate, prepared without ſuperfluous ſteam, at the interval of two days.

——	——	Diſtreſſed the moment of immerſion.
0	20	Scratched the veſſel furiouſly.
0	25	Fell on its ſide.
0	35	Motionleſs and inſenſible—taken out.
——	——	Lay as dead ſome time; finally recovered,

Another rabbit of the ſame brood, (before immerſion in water, viſibly much affected with fear) ſtruggled with ſtrength for a minute and an half. At the end of two minutes, forty ſeconds, it moved: in 3 minutes was taken out, but did not recover.

Should theſe experiments be repeated by a perſon, careful to procure his elaſtic fluids free from offenſive acid fumes, the diſtinctneſs of the phaenomena I obſerved, perſuades me that their general reſult will be confirmed. Of ſome readers, whom the importance of the ſubject may lead to take up this pamphlet, the curioſity will, I fear, be repreſſed by ſenſations, ariſing from the idea of pain endured by the animals. In a few caſes, the torture which was inflicted was exceedingly repugnant to my own feelings; and for this reaſon, I have left one ſeries of experiments (SECT. VI.) more incompleat than I could eaſily have rendered it. Againſt drowning, an imputation of cruelty will hardly lie: Animals, deſtined to this death, may juſt as well drown for the inſtruction of the phyſician. Beſides, did not accuſtomed acts of outrage and injuſtice daily paſs uncenſured, I know not how he who feeds upon the fleſh of a ſlaughtered animal can, upon reflection, condemn inveſtigations, ſeriouſly tending to reſtore or preſerve health, though conducted at the expence of the life and eaſe of animals, unable to reſiſt the power of man. I wiſh, with all my heart, I could prove that moroſe writer in the wrong, who has called the Earth A VAST FIELD OF BATTLE, where creature, for preſervation, preys upon creature, or tortures its fellow in purſuit of pleaſure.

Two kittens immerſed, one in carbonic acid, the other in hydrogene air, afforded a ſimilar reſult; that is, the carbonic acid appeared full three times as deleterious as hydrogene.

Finally, to render the difference again more diſtinct, two equal quantities of atmoſpheric, were ſucceſſively mixed with an equal bulk of carbonic acid, and of hydrogene, air. A rabbit S being put into the mixture of atmoſpheric and carbonic acid air; the following obſervations were made.

	Minutes.	
In	2	Appeared weak.
	4	Has been couchant for 2 minutes.
	6	Very ſtill.
	11	Reſpiration more laborious.
	26	Extremely weak; ſeems ready to fall on one ſide; ſcarce alive.

43 Quite dead.—After the 2d minute it never rose—death very lingering.

A fellow rabbit, T, in atmofpheric and hydrogene air, feemed much lefs diftreffed at firft; rubbed its fore-feet after it had continued in the veffel 40 minutes, and performed feveral other actions; much of the time it fate, that is, it continued erect before. Even at the laft, no diftrefs, except quick refpiration, was obfervable.

In 48 minutes it was taken out; it now ftood firm; and though unwilling to move, was capable, when urged forward, of advancing, without ftaggering, or any fign of great debility. In appearance it had fuffered lefs in 48 than its fellow in 15 minutes.

Reflections on the preceding facts.

The attentive reader muft have feen, even in the refult of thefe fimple extemporaneous experiments, indubitable proofs of the power of factitious airs to affect the living frame. It appears that oxygene air, when infpired pure, or nearly fo, increafes all the internal motions fo as to produce dangerous or mortal inflammation; that by reddening the blood, it brightens the colour of the folid parts; even that of the liver, which anatomy fhews to be the leaft likely of all the folids to be affected by any change of the arterial blood: that it renders animals lefs capable of being drowned or deftroyed by cold; that it is expended in mufcular motion, fince animals that have exerted themfelves violently, immediately before confinement in a given quantity of atmofpheric air, or during confinement, fooneft exhauft it of oxygene; and that, when it is blown into dogs, in the manner veal is blown by butchers, it produces a remarkable degree of vivacity. Thefe facts, compared with fome of the obfervations, which will be given in the next paragraph, will prove of ufe in directing us how to apply this air properly as a remedy.

Between the unrefpirable airs, there feems a remarkable difference in their power to produce infenfibility and death. Hydrogene appears the leaft noxious, both when infpired alone, or mixed with atmofpheric air. Azote probably differs little from hydrogene. Hydro-carbonate feems extremely deleterious; Mr. Watt gives evidence

 of

of this in the human ſpecies. I can add a ſimilar obſervation. A perſon in confirmed conſumption breathed a quantity of hydrocarbonate, mixed with 4 times its bulk of atmoſpheric air: he became very ſick, or rather vertiginous; the pulſe was much quickened, and the extremities became very cold. The patient finding an abatement of pain in his ſide, and of dyſpnoea, returned for another doſe. The perſon who prepared this air, is one of the moſt ſkilful chemiſts, and moſt celebrated mechanical philoſophers in Great Britain. Thinking the former doſe too ſtrong, he now mixed 50 c. inches of hydrocarbonate with 600 of atmoſpheric air. This was reſpired without any ſenſible effect. In a quarter of an hour, 100 c. i. of hydrocarbonate were mixed with 600 of atmoſpheric air. The patient breathed at twice about two-thirds of this mixture, when he was deſired to deſiſt. Soon afterwards he became vertiginous and nearly inſenſible, his pulſe at one period being nearly imperceptible; the ſphincter of the bladder was relaxed; after his recovery, he was again very cold, "intenſely cold to his own feelings" was his expreſſion, as well as to the touch. After getting into his carriage, he fainted; and his pulſe for ſeveral hours continued quicker and weaker than before. The operator having obſerved, that when much water is added to red-hot charcoal, carbonic acid air is copiouſly produced, in the preparation of this laſt portion of air, had added ſo little water, that no ſuperfluous ſteam at all came over; hence it was as pure as can be made: being alſo newly prepared, it retained all the charcoal it had carried up; of which it is well known to depoſit part on ſtanding. This leads me to conjecture, that the greater deleterious power of heavy inflammable air from water and hot charcoal (hydrocarbonate) compared with that of light inflammable air, depends on the facility of its combination, or at leaſt of the charcoal it contains with the oxygene of the blood; in conſequence of which, it ſpeedily diſarms the ſyſtem of its moving principle, and induces lifeleſsneſs. This opinion is countenanced by the effect of nitrous air, which more quickly deſtroys life than any of thoſe above-mentioned, and which is well-known very readily to combine with oxygene.

Inſtantaneous

Inſtantaneous death, in this caſe, may be imputed to the inſtantaneous production of an highly corroſive acid (nitrous acid) and its application to the whole ſurface of the lungs. And this hypotheſis may be thought to be corroborated by the *rapid* effect of carbonic acid air in occaſioning death, for which I confeſs myſelf unable to aſſign any plauſible chemical, or phyſiological reaſon. In the mean time, the facts I have related, oblige me to reject the opinion of thoſe eminent philoſophers, who have of late ſuppoſed that water and ſeveral bland unreſpirable airs produce death, ſimply by excluſion of the oxygene of the atmoſphere. Whether it be that ſome rob the body of this principle, and others exhauſt the ſenſorial power by an unknown operation, their action is certainly unequal; and I preſume, recovery from aſphyxia in water (when but little goes down the wind pipe), hydrogene air, azote, or from ſtrangulation (where no material injury from violence is produced), will be much more eaſy than from aſphyxia, occaſioned by other unreſpirable mediums.

Experiments to diſcover the effects of the long continued action of aeri-form ſubſtances, would be much more curious than ſuch as I have made. They would thus, in all probability, more deeply and permanently affect the living ſyſtem. If, for inſtance, an animal were kept in an atmoſphere containing $\frac{20}{100}$ – $\frac{24}{100}$ of oxygene or ſtill leſs, it would perhaps be affected by the ſea ſcurvy. Again, if three equal growing animals were kept, one in the atmoſphere, the other in air of an higher, the third of a lower, ſtandard, and in all other reſpects treated alike; ſome conſiderable difference would perhaps be obſerved in their growth and vigour. By frequent immerſion in water, the aſſociation between the movements of the heart and lungs might perhaps be diſſolved; and an animal be inured to live commodiouſly for any time under water. If any plan, ſimilar to that of which I have attempted the outline, ſhould be executed, ſuch proceſſes of inveſtigation ought to be carried on in the Inſtitution.

XI.—*Some effects of the inſpiration of hydrogene, to elucidate the reſult of the foregoing experiments.*

" When an animal is immerſed in water, *his pulſe becomes weak and frequent*, he feels an anxiety about his " breaſt,

" breaſt, and ſtruggles to relieve it: in theſe ſtruggles, " he riſes towards the ſurface of the water, and throws " out a quantity of air from his lungs. After this, *his* " *anxiety encreaſes, his pulſe becomes weaker*; the ſtruggles " are renewed with more violence; he riſes towards the " ſurface again; throws out more air from his lungs, and " makes ſeveral efforts to inſpire; and in ſome of theſe " efforts, a quantity of water commonly paſſes into his " mouth; *his ſkin then becomes blue, particularly about* " *the face and lips*; *his pulſe gradually ceaſes*; *the ſphinc-* " *ters are relaxed;* he falls down without ſenſation, " and without motion" *(Dr. Goodwyn, l. c. pp* 3, 4.*)* This deſcription of drowning in water applies, as far as the circumſtances admit of compariſon, to the effects occaſioned by the reſpiration of pure hydrogene. I have remarked them in a number of healthy perſons, who were curious to try how long they could breathe this air. The frequency and debility of pulſe, blueneſs of the lips and coloured parts of the ſkin, were always obſervable in a minute, or a minute and an half. Beſides, dizzineſs was felt, and the eyes have grown dim; in animals, the tranſparent cornea has appeared ſunk and ſhrivelled. Several individuals agree in deſcribing the incipient inſenſibility as a ſtate highly agreeable. One conſumptive perſon loved to indulge in it; for this purpoſe contrary to my judgment, he uſed to inſpire a cubic foot of hydrogene at a time. This quantity moſt commonly produced little change in his feelings. Sometimes it brought on almoſt compleat aſphyxia. During this proceſs, I have felt the pulſe nearly obliterated. Afterwards, as he recovered, it was ſenſibly fuller, and ſtronger than before the inſpiration. This fact belongs to a general principle now beginning to be underſtood; when the ordinary powers have been, for a certain time, withheld from the body, they act with greater effect, as holding the fingers to the fire after handling ſnow, occaſions ſevere aching. For this reaſon, whenever air with leſs oxygene is to be inſpired, it would ſeem more advantageous to employ for a long time an atmoſphere little reduced, than one ſo low that it can only be breathed for a ſhort time.

An obſervation the patient juſt mentioned, made upon himſelf, ſeems to ſhew the neceſſity of oxygene to muſcular action. Judging from his feelings, that he was perfectly recruited after his doſe of pure hydrogene, he has riſen from his ſopha with an intention to walk about his apartment, but has been ſurpriſed on riſing, to find himſelf incapable of advancing three ſteps, till he had reſted ſome time longer. In this caſe, was not the ſtore of looſely combined oxygene, laid in before, expended during the inſpiration of the hydrogene, by thoſe motions which are perpetually going on in the ſyſtem ? Did it not require ſome time to replace the neceſſary portion in the muſcles, remote from the heart and lungs ?

XII.—*Some particulars relative to oxygene, ſupplemental to the preceding experiments.*

The celebrated Dr. Ingenhouſz in a letter dated Auguſt 4th, 1794, mentions to me a very curious experiment, " which," ſays he, " if it be a real fact, throws " a great deal of light upon your ſyſtem ; it is this :— " Bliſter your finger, ſo as to lay bare the naked and " ſenſible ſkin. The contact of air will produce pain : " put your finger into vital air, and this will give more " pain ; introduce it into fixed or azotic air, and the " pain will diminiſh or ceaſe." Dr. Webſter, he adds, was informed of theſe circumſtances, by a Frenchman, whoſe name does not appear ; I had often heard them indiſtinctly related ; and it is rather ſurprizing that the fact has not been aſcertained. Much of the art of modern ſurgery conſiſts in keeping the air from wounds and ſome kinds of ulcers : and this fact, if the account be true, pretty deciſively ſhews which ingredient of the atmoſphere is injurious.

I applied a bliſter an inch long, and half an inch broad, to the back of the third finger of the left hand. When the pain from the action of the cantharides had entirely ceaſed, I cut away the ſcarf-ſkin of the veſication ; and was ſenſible, the moment the air was admitted, of a ſharp ſmarting pain. This did not continue ſo ſevere ; but the expoſed true ſkin ſenſibly ſmarted. Upon tying the neck of a bladder, containing carbonic acid

acid air from heated chalk, round the root of the finger, the pain very soon subsided. While I kept my finger in carbonic acid air, which was near half an hour, I should not have known it had received any injury. On taking it out, the surface had a whitish appearance—Was this from the beginning of the formation of epidermis?—In the air—the experiment was made in a warm temperature—the smarting returned; in an hour the exposed skin was painful and looked angry, as the expression is: I again inclosed it in carbonic acid air; in six minutes I felt no more pain. After several hours I again removed the bladder, and soon felt the smarting return.

During the hour after my finger had been for the first time taken out of the bladder, I had introduced it into a phial of oxygene air, for a few minutes, but was not sensible of increase of pain; nor can I say that the redness and angry appearance was owing to this circumstance.

At Oxford, in 1790, I had proposed to a distressed negro, to try to whiten part of his skin with oxygenated marine acid air. He was to exhibit the appearance, if it should be curious, for the relief of his family. His arm was introduced into a large jar full of this air, and the back of his fingers lay in some water impregnated with it at the bottom of the vessel. It was perceived that he had ulcerations from the itch between his fingers; and this made me very cautious about the experiment. In 12 minutes he complained of severe pain from the ulcers, and the arm was withdrawn. The back of his fingers had acquired an appearance as if white lead paint had been laid upon them, but this did not prove permanent. A lock of his hair was whitened by this acid.—Next day the ulcers became extremely painful, and the hand swelled from the inflammation; this deterred him a continuance of the experiment after he was cured of his complaint. You cannot safely impute the effect of this powerfully stimulating acid to its oxygene alone.

But the fact stated by Dr. Ingenhousz is very agreeeable to the common phænomena presented by wounds. Moreover, I have lately seen cancerous patients treated by the application of unrespirable air, with the most astonishing success. In mentioning to Dr. Black the introduction

introduction of factitious airs into the BATH hospital, as a source of hope, I did not so soon expect an event which ages and nations have desired in vain. Observations, extremely analogous to the result of my single experiment with carbonic acid air, were there made during the course of the treatment, of which the physician who employed it will speedily give a particular account to the public. Should it be invidiously observed by any reader of his narrative, that something similar had been tried before, it may be truly replied, that these trials were rather discouragements to the new application of elastic fluids; and that failure in former instances enhances the merit of the recent method.

While this section was printing, I made the following experiments: The raised epidermis of a blistered finger, after all action of the cantharides had ceased, was cut away in carbonic acid air. No pain was felt. The atmospheric air slowly mixed with the other in the glass cylinder, as I found by the dull manner in which a candle burned in it; and now some slight pain was felt. The finger being put into oxygene air, a smarting came on, and lasted 20 minutes; but now became less. The finger was next put into air containing alkaline fumes; and the pain was much severer than ever. A 2d blister being opened in the air, smarting pain came on. In a bladder of fixed air it soon went off. The epidermis was cut off from a blister on my own finger, which I instantly plunged into oxygene air; it felt as when salt is sprinkled on a cut: and the pain was, I am pretty sure, more severe than when my former blister was opened in the atmosphere. In carbonic acid air the pain in two minutes quite subsided, and returned when I exposed the bare skin to the atmosphere.

XIII.—*Of the preparation of atmospheres of different standards.*

Perspicuity in the directions, which cannot for all readers be attained in *reasonings*, being a principal object in the present pamphlet, it may be useful, before I proceed, to exhibit a view of those mixtures which furnish atmospheres of an higher or lower standard, than the common

air. By *an higher ſtandard*, I mean more than 28 parts of oxygene in 100; by *a lower ſtandard*, leſs. For the ſake of brevity, we might ſay, *air of the ſtandard of thirty ſix*, inſtead of "air containing thirty-ſix parts of oxygene in an hundred parts."

Mr. Watt's hydraulic bellows furniſh the means of throwing any proportions you pleaſe of the different airs into a common reſervoir. The effect, as far as can be ever uſeful in practice, is ſhewn in the following tables:

Change of the ſtandard of atmoſpheric air, by addition of other airs.

The ſtandard of atmoſpheric air being 28 oxygene, 72 azote, it is altered in this manner, by the addition of ſucceſſive equal parts of atmoſpheric to one of oxygene:

		Oxygene.	Azotic.
1 part of atmoſpheric	to 1 of oxygene -	64	- 36
2 of atm. -	to do. - - -	52	- 48
3 do. - -	to do. - - -	46	- 54
4 do - -	to do. - - -	42	- 58
5 do. - -	to do. - - -	40	- 60
6 do. - -	to do. - - -	38	- 62
7 do. - -	to do. - - -	37	- 63
8 do. - -	to do. - - -	36	- 64
9 do. - -	to do. - - -	35	- 65
10 do. - -	to do. - - -	34½	- 65½
11 do. - -	to do. - - -	34	- 66
19 do. - -	to do. - - -	30½	- 69½

N. B. Small fractions are neglected.

The ſtandard is altered in the following manner, by addition of ſucceſſive equal parts of oxygene to one of atmoſpheric air:

		Oxygene.	Azotic.
2 oxygene -	to 1 atmoſpheric -	76	- 24
3 oxygene -	to do. - - -	81	- 19
4 do. - -	to do. - - -	85	- 15
5 do. - -	to do. - - -	88	- 12

Reſpecting theſe two tables, it is to be obſerved, that the moſt ſkilful chemiſts have never been able to obtain oxygene

oxygene air quite pure; it may therefore be allowed, that in ſuch as will commonly be prepared, not more than 80 or 85 parts in 100 will be pure oxygene; unleſs it be prepared from good manganeſe and rectified vitriolic acid; of this waſhed in lime-water, ſcarce 10 parts in 100 will be unreſpirable. The unreſpirable air, with tolerable care, will be obtained free from oxygene. The following proportions, therefore, will be more exact than the foregoing:

Effect of the addition of different portions of atmoſpheric to one of unreſpirable air.

		Oxygene.	Unreſpir.
1 atmoſpheric -	to 1 unreſpirable -	14 -	86
2 do. - -	to do. - - -	19 -	81
3 do. - -	to do. - - -	21 -	79
4 do. - -	to do. - - -	22 -	78
5 do. - -	to do. - - -	23 -	77
6 do. - -	to do. - - -	24 -	76
7 do. - -	to do. - - -	24 -	76
8 do. - -	to do. - - -	25 -	75
9 do. - -	to do. - - -	25 -	75
10 do. - -	to do. - - -	$25\frac{1}{2}$ -	$74\frac{1}{2}$

Effect of the addition of different portions of unreſpirable airs to one of atmoſpheric.

1 atmoſpheric -	to 2 unreſpirable -	9 -	91
1 do. - -	to 3 do. - - -	7 -	93
1 do. - -	to 4 do. - - -	$5\frac{1}{2}$ -	$94\frac{1}{2}$
1 do. - -	to 5 do. - - -	5 -	95

It ſeems not improbable, that on certain ill-conditioned ulcers, oxygene has a ſalutary effect, by occaſioning greater action, both of the veſſels which throw out the copious thin diſcharge, and of the abſorbents. Many ſubſtances, uſually applied to ſuch ulcers with ſucceſs, as metallic ſalts, contain much oxygene, and ſome are moſt highly charged with this principle, as the red oxyds of metals. The following intelligence, if authentic, adds confirmation to this opinion, and may prove uſeful: A few months ago, I was ſtruck with the frequency of ſcrophulous tumors among the poor of the county of Long-

ford, in Ireland. Suppofing that neceffity might have occafioned the trial of many methods of cure, I enquired whether the people there had not fome peculiar domeftic practices in fuch complaints. A phyfician referred me to a fimple but very reputable old farmer, as remarkably fuccefsful in fcrophulous fores. With this perfon I had an interview. In his practice, he had no view to gain; and that, in his principles, he had nothing of empirical impofture, he convinced me, by at once difclofing all his art. He had himfelf, many years ago, an ulceration of the fubmaxillary glands: This, after various unfuccefsful applications, was healed by a ruftic practitioner like himfelf. He obtained a knowledge of the remedy, by which, during a long life, he affured me he had himfelf healed many fuch ulcers of the glands about the jaws. He was fo little fpeculative, as never to have attempted the cure of an obftinate fore in any other feat. That he might effectually inftruct me, he brought fpecimens of his fimples. They were the leaves and ftalks of wood-forrel *(oxalis acetofella)*, and the root of meadow-fweet *(fpiraea ulmaria)*. The forrel he prepares by wrapping it in a cabbage leaf, and macerating it by its own juices in warm peat afhes. This pulp is applied like a poultice to the ulcer, and left 24 hours; the application of forrel is four times repeated; then the roots of the meadow fweet, bruifed and mixed with the four head or efflorefcence that appears on butter-milk, left in the churn, are ufed in the fame manner till the fore heals, which I was told always fpeedily happens, often in 2 or three weeks.

The following extract of a letter from Mr. Edgeworth, of Edgeworthftown, contains fome fupplementary information, and will probably add fo much to the credit of my information, as to obtain a trial for the remedy.

" I have learned from Mr. Mills, that when he was about eight and twenty, he had two large fcrophulous fwellings in his neck, one under each ear, near the jaw; the marks they had left he fhewed me. He was attended by a furgeon in the neighbourhood for fome weeks, without receiving any benefit. A farmer, with whom he was acquainted, recommended the application he mentioned to you, by which he was completely cured. The man told him the names and quantities of the feveral ingredients,

dients, when he applied them, but did not till ſome years afterwards (when he was leaving this country for America) apprize him that the *myſtery of the cure* (that was his expreſſion) depended entirely upon the ſorrel. This perſon had predicted to Mills, that one of the ſores, which had been lanced, would not heal ſo ſoon as that which had ſuppurated of itſelf; and he found this to be true. Whilſt he had ſcrophulous ſwellings, he was weak and unhealthy; from the time the wounds were healed, he has been ſtrong and active; he is now eighty; and whilſt he was relating theſe circumſtances to me this evening, he kept pace with my horſe up hill for half a mile, without any apparent effort. I mention this, becauſe it is a common opinion (I ſuppoſe a vulgar error) that healing ſuch ſores is prejudicial to the general health. He has applied this remedy to upwards of an hundred different perſons, every one of whom have been cured.— Seven years ago I remember having ſeen his ſon, who rents a conſiderable farm from me, with an enormous ſcrophulous ſwelling on his neck; he was in great pain, was weak, and emaciated; he was too impatient to wait for a ſuppuration of the ſwelling, and would have the plaiſter applied to it whilſt it was unbroken; the cure was protracted, but was effectual; he has had no return of the complaint; a ſlight inequality of ſurface ſtill remains upon his neck. Mr. Mills has communicated his recipe to ſeveral, and in particular to a very intelligent perſon in this neighbourhood, who has employed it with unfailing ſucceſs. All the patients complain of the ſeverity of the application; and in every ulcer to which it is applied, there takes place a remarkable change from a dead pale to a bright ſcarlet colour." *July* 17, 1794.

This change of colour indicates communication of oxygene, which perhaps the oxalic acid of the ſorrel contains in ſuch a ſtate of combination as eaſily to part with a portion. Now Dr. Darwin, in his ZOONOMIA, attributes ſcrophulous ſwellings of the glands to inirratibility, which, as I have conjectured, may ariſe from a certain deficiency of oxygene. Theſe principles would ſupply an obvious theory, were we but certain of our facts. If however, as the preceding account implies, ſorrel produces detumeſcence of the glands before ſuppuration, its application

application will be, I suppose, a more eligible practice than any now in use. Writers in the Materia Medica may have applied *deobstruent*—their word of course—to this plant, but I remember no particular commemoration of its virtue in scrophula. Murray, a compiler of extensive reading, has nothing to this purpose.—(*Apparat. Medicam.* III. 492-9).

XIV.—*Of the method of procuring elastic fluids.*

To procure a dose of factitious air by means of Mr. Watts' apparatus will, I think, be found quite as easy as to dress a joint of meat. In two instances under my eye, a servant of good plain understanding, has managed the apparatus perfectly. One failed at first for want of making the charge in the cast iron pot red hot, before he let the water drop. Hence he got steam instead of air. When the joints are made tight, and the heat is proper, and the water does not drop too fast, the operation proceeds perfectly well. Mr. Watt gives a sufficient variety of lutes. A strip of oiled silk bound fast round a joint, alone makes a good lute and will bear much heat.

I was for some time anxious concerning oxygene air. Expecting this would be full as extensively useful in medicine, as any unrespirable air, I wished for a method equally simple of procuring it. Knowing that the manganese from the Mendip hills gives 1. azotic, 2. oxygene, 3. azotic with carbonic acid air; so that the whole product is not much superior to the atmosphere, I feared lest inexperienced operators should be incapable of catching the best part of the produce. I therefore, at the suggestion of Mr. Hermbstaedt and Mr. Chaptal turned my attention to the solution of manganese in vitriolic acid. Mr. Hermbstaedt had found a pound of the Ilefeld or Ilmenau manganese, with strong vitriolic acid, to yield 3384 cubic inches of "the best oxyygene air."—(*Hermbstaedts Versuche*, B. II. p. 49.) Mr. Chaptal obtained full as much from French manganese. I procured 150—200 c. i. of oxygene air (which by the nitrous test proved excellent) from oil of vitriol and 1 oz. Exeter manganese. But when I lately came to make experiments with a view to discover a proper method for common practice, I perceived that this process was highly objectionable.

objectionable. The firſt portions of air procured by means of the oil of vitriol of commerce contained much oxygenated marine acid air—a ſpecies of elaſtic fluid exceedingly deleterious and irritating to the lungs. This happened becauſe ordinary oil of vitriol is contaminated with muriatic acid. Beſides, as the acid of vitriol will itſelf be carried up by the heat neceſſary to extricate the air by this operation, the veſſels will ſuffer from corroſion, unleſs troubleſome precautions are employed. The air itſelf too will not eaſily be totally freed from the pernicious acid fumes. Hence, contrary to my firſt intention, I ſhall omit directions for procuring oxygene air from oil of vitriol and mangaueſe; they are further become unneceſſary, ſince Mr. Watt's apparatus anſwers perfectly well for this alſo, according to his laſt directions. The ſkilful chemiſt, who may chooſe to procure it in this way, will be ſufficiently apprized of the neceſſary precautions. For others it is added, that the Exeter mangaueſe is in no reſpect preferable to any other, which does not contain much calcareous earth, or ſome noxious mineral, which latter is not the caſe with any manganeſe I know. To diſcover whether the manganeſe you are about to uſe contains calcareous earth, pound it coarſely, and put it into a glaſs containing a little water. When it is moiſtened, add ſome nitrous acid. If a conſiderable working or effervefcence takes place, the manganeſe contains calcareous matter. Strong vinegar will do tolerably well inſtead of nitrous acid; ſo will weak vitriolic acid. But ſpirit of ſalt or muriatic acid is not proper. Examined in this way, the Mendip manganeſe effervefces much, the Exeter not at all.

Should you wiſh to procure for breathing carbonic acid air by ſolution of chalk, I would adviſe the uſe of vinegar inſtead of vitriolic acid. When the chalk is coarſely pounded, ſo as to leave no bits larger than peaſe, you will have a continued effervefcence from common vinegar mixed with about an equal bulk of water. You will avoid thoſe offenſive ſulphureous fumes which vitriolic acid gives out. If Nooth's glaſs apparatus is uſed upon ſuch occaſions, put a pint of water in the middle veſſel, and add to it half an ounce of ſalt of tartar or of ſoda.—To impregnate hydrogene air

with

with zinc, I have thought it sufficient to put a few ounces of zinc (which in the shops is called *speltre*) into the pot, the rest of the charge being of iron.

XV.—*Hints for the use of unrespirable airs.*

I cannot pretend to lay down any rules, founded upon large experience, for the employment of unrespirable airs in medicine. It was not in my power to administer these in quantity till late in June 1793. It could not be expected that great numbers would immediately have recourse to a new method, not recommended by any of those arts by which the credulous are deluded; and I had no hospital at command. Of the interval (since the middle of 1793) part has been spent in absence, and part employed about the means of putting the public in possession of such an air apparatus as could be generally used. I have, however, acquired the knowledge of some facts, and the right of giving some cautions.—One part of hydrogene (and probably of azote) may be freely employed from the first, with three or four parts of atmospheric air. Of this mixture suppose the patient to inspire for a minute; then to attend to his feelings. By degrees he may prolong the time of inspiration, and increase the proportion of hydrogene; but should not in any chronic complaint use more than an equal portion of hydrogene. In that class of diseases, where a diminution of action is required, these airs may be used with high probability of advantage, particularly where the lungs are inflamed. I have seen a considerable inflammation of the lungs, with pain, full pulse, and flushed countenance, instantly relieved by a mixture of one part of hydrogene, and about six parts of atmospheric air, inspired for a quarter of an hour. One of my friends, who, I hope, will publish the particulars, administered air of a reduced standard to a child, violently affected by the croup. The child was at first unwilling to inspire; but being prevailed upon, his countenance soon manifested the relief he experienced, and his skin felt cooler, while he was inspiring. The disease from this time abated; and after a repetition of the means, the child recovered. Phthisical patients, even such as have been in the last stage, have many times attested to me, that hydrogene mixed with common air allayed their

their diſtreſſing heats. I have reaſon to believe, that by inſpiring this mixture once or twice after going to bed, the night fever and ſweat may be prevented. For this purpoſe, the patient ſhould take from half a cubic foot to a cubic foot of hydrogene mixed with common air, before he falls aſleep, and after awaking for the firſt time.

In the opinion I formerly advanced concerning the ſoporific virtue of a reduced atmoſphere *(Obſervations on Calculus, p. 80)*, I am ſomewhat confirmed by ſubſequent occurrences. A perſon in conſumption, who, for months, had taken opium at night, on breathing hydrogene freely, could ſleep perfectly well without opium. He remarked, that his ſleep was more profound than uſual. The air of his room being largely mixed with hydrogene, his ſervant neceſſarily inſpired much of it. The man had been accuſtomed to reſt ill; and now ſpontaneouſly remarked, "that he did not know what was "come to him, he ſlept ſo ſound."

At firſt I have thought hydrogene air to impair the appetite. But the effect has not been permanent. Hydrogene from ſolution perhaps produces nauſea by its diſagreeable ſmell.

Carbonic acid air ſhould not be uſed at firſt in greater proportion to atmoſpheric than from $\frac{1}{9}$ to $\frac{1}{6}$. I have been ſurpriſed that this acid air ſhould have proved ſo grateful to the lungs. I have never ſeen or heard of its exciting pain or coughing in confirmed conſumption.— It has even inſtantaneouſly relieved phthiſical dyſpnoea in ſeveral caſes, which other unreſpirable airs have done alſo. The facts related in Sect. XII. will perhaps help to ſolve the difficulty concerning the mild action of carbonic acid in ulcerated lungs. This acid air, during the day, and hydrogene at night (to prevent fever), I think, may be uſefully combined.

The uſe of hydrocarbonate air requires repeated precautions. Though this appeared not at all more ſuddenly deleterious than carbonic acid to animals immerſed in theſe fluids, it produces a much more powerfully depreſſing effect, when inſpired in mixture with atmoſpheric air: 100 cubic inches of hydrocarbonate with 1200 of atmoſpheric, breathed at ſhort intervals, have ſenſibly

weakened patients, who felt no ſuch effect from 100 of carbonic acid, with 600 of atmoſpheric, air. The extremities have been remarkably chilled by the firſt mixture. The debilitating power of hydrocarbonate is ſo extraordinary, that it may perhaps be the far-famed miaſma of the marſhes. *This air ſhould not at firſt be inſpired with leſs than* 12, *or perhaps* 15, *times its bulk of common air.* A medical gentleman, upon witneſſing ſome of its effects, imagined it would be particularly uſeful in the ſtrangulated hernia. In this moſt painful and dangerous diſorder, any unreſpirable air, given ſo as to produce ſome vertigo and faintneſs, promiſes to be uſeful. Though, according to the ſcholaſtic adage, *it is eaſy to add to inventions*, and purſue analogies, I have ſingled out this idea as not extremely obvious.

No cautious thinker can, without long experience, feel aſſured that he has been highly ſucceſsful in preventing imminent conſumption. A lowered atmoſphere has, however, evidently appeared to me to effect this where common remedies afforded little hope. To one ſtate, previous to conſumption, this remedy, judiciouſly managed, ſeems peculiarly applicable. It is where the lungs are affected with a ſpecies of inflammation, and yet the patient is ſo reduced, that you can ſcarce venture upon debilitating medicines, and where even the ſtricteſt diet does more general miſchief than local good.

Typhus, when combined with pneumonic ſymptoms, is a diſeaſe of the utmoſt danger. In the hoſpitals, among a number of patients, ſometimes ſcarce one ſhall recover. May not the pneumatic practice ſtand us in good ſtead here? that is to ſay, the cautious uſe of a lowered atmoſphere, while ſtimulants are freely given by the mouth.

XVI.—*Of the employment of oxygene air in diſeaſes.*

One principal argument for trying unreſpirable airs in conſumption, is the experienced detriment ſuſtained from the uſe of oxygene. I have given an abſtract of 20 caſes, where Dr. Fourcroy has deſcribed it as ultimately prejudicial (*Obſervations, p.* 126, 127). Dr. Scherer of Vienna, has ſince atteſted the ſame thing. I had every reaſon to confide in the accuracy of Fourcroy, and in

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one inſtance have ſeen a clear confirmation of his teſtimony. In Auguſt 1793, a patient, unconquerably diſguſted at hydrogene air, becauſe it produced ſome dizzineſs and nauſea on the firſt trial, entreated me to ſuffer him to inſpire a little oxygène. He obſerved, that as I believed this to be pernicious only from theory and the aſſertions of others, I ought to afford him what he conceived to be a chance of recovery. He breathed twice a day, for 5 minutes, a mixture of 1 part oxygene, to 3 parts atmoſpheric, air; at the moment he ſaid he felt lighter in his cheſt; but in 2 days, the cough became harder, the fever more violent, and all the ſymptoms were evidently aggravated. Believing that he had taken cold (as conſumptive perſons are ſo apt to perſuade themſelves when their diſorder becomes more ſevere), he tried an higher atmoſphere, after ſome interval, again. The effect was the ſame.

Another reaſon for removing oxygene from the air inſpired by ſuch patients, as much as poſſible, is its pernicious action upon moſt ulcers; and as it would ſeem on the ulcers of their lungs. Whether or not, in ſome caſes of conſumption, or of the indiſpoſition previous to conſumption, any exceſs of oxygene is received into the body, as I conjectured from certain appearances, I willingly leave to future obſervations to decide. If the hypotheſis be juſt, it ſuggeſts an additional argument for uſing a lowered atmoſphere.

There is, however, one ſtate of diſeaſed lungs, often followed by ſpontaneous ulceration, in which oxygene air promiſes to be uſeful. This ſtate is mentioned in ZOONOMIA, p. 300. It is attended with much expectoration of mucus, which the author, with much probability, imputes to want of irritability in the pulmonary abſorbents. This defect, eſpecially in pale perſons, who are threatened with that variety which has been called the *pituitous* conſumption, air of an higher ſtandard may cure. To prevent relapſes in hydrothorax, by rendering the abſorbents of the cellular ſubſtance of the lungs more irritable, oxygene ought to be employed.

Aſthma is a diſeaſe in which we might expect benefit from modified air. I am not now, however, ſo ſanguine in my expectations from an atmoſphere of an higher

 ſtandard,

ſtandard, in all inſtances of this diſeaſe (the ſpaſmodic aſthma). In one caſe, on repeated trials under my own cautious management, ſuch an atmoſphere repeatedly induced ſtricture of the thorax, and at night ſeverer paroxyſms. The patient requeſted me to ſatisfy myſelf fully, before I relinquiſhed this mode of trial. He then deſired to inſpire a lowered atmoſphere, which certainly, upon a few trials, did not induce ſtricture or aggravate his complaint. But circumſtances ſeparated us before this method was fully tried. The following reaſoning of Dr. Darwin alſo induces me to hold up a lowered atmoſphere to the notice of thoſe who have aſthma to treat. "If "the excitability of the ſyſtem depends on the quantity "of oxygene abſorbed by the lungs in reſpiration, ſleep-"ing in an atmoſphere with leſs oxygene, might be of "great ſervice in epileptic caſes, and in cramp, and even "in fits of the aſthma, where their periods commence "from increaſe of excitability during ſleep."

In another caſe of aſthma, where an higher atmoſphere was uſed from my recommendation, but not under my inſpection, the patient aſſured me he felt great relief; and even when he was imprudent enough to breathe oxygene air undiluted, his aſthma was not rendered more ſevere; though, for a time, he was affected in a very curious manner: The ends of his fingers, in particular, ſwelled, and were very ruddy. After ſome uſe, his very imperfect apparatus failed: and the treatment was diſcontinued for want of air. I relate theſe perplexing appearances as difficulties to be ſolved by future obſervation. They may ſerve alſo as precautions.—In ſome diſeaſes, where the patient has been affected with languor and debility, oxygene air has appeared highly ſerviceable. It has been uſed in chloroſis with ſpeedy good effect. In coldneſs of the extremities of weak and old people, I have ſeen it remarkably efficacious. It raiſes the ſpirits, and, as far as I have been able to obſerve, without the ſubſequent or indirect debility occaſioned by opium or alcohol: A medical friend once obſerved, upon ſeeing a perſon who had been ſome time reſpiring diluted oxygene air, "his blood ſeems to flow in veſſels it never pervaded before." This had much the appearance of being the caſe. If ſo, ſuch an atmoſphere may retard the

the approach of old age, when many veſſels are ſuppoſed to cloſe up. In illuſtration of the effect of oxygene air on a perſon advanced in life, that is, between 50 and 60, with extremities habitually cold, I ſubjoin a note from Dr. Barrow, of the Hotwells, who lived in the ſame houſe with him. " —'s pulſe increaſed in frequency and " ſtrength; there were alſo other unequivocal ſigns of " general excitement; ſuch as univerſal ſenſation of " heat, and an aſtoniſhing increaſe of animal ſpirits, evi- " denced not only in his converſation, but his general " conduct. His ideas ſeemed to run altogether on ſub- " jects of juvenile gratification. His tricks and geſtures " afforded the company much entertainment. This " could not be aſcribed to any change in his diet; for he " had at this time diſcontinued his uſual quantity of wine " and the ſtimulating gums he had been accuſtomed to. " I am not ſure whether he had the head-ach, and other " ſymptoms of debility, ſubſequent to a debauch with " wine. Your's, &c.

" W. Barrow."

This perſon breathed a mixture of one part of oxygene, with 4 parts of atmoſpheric air, twice a day, at firſt for 10, and then for 20 minutes.—I have had two patients who breathed an higher atmoſphere for every day more than 6 weeks each. The oxygene was procured from Mendip manganeſe. Theſe two inſtances, joined to my perſonal experience, ſeem to ſhew that there is no noxious impregnation in that mineral. The one was ſubject to great difficulty of breathing with exceſſive expectoration of mucus, from bad conformation of the thorax apparently. He was always much relieved by this modified air, but not radically cured. The other had encyſted dropſy with anaſarca of the lower extremities, and (as I ſuppoſed, from the difficulty of breathing, irregularity of the pulſe, and other ſymptoms) of the lungs alſo. By gradually heightening the ſtandard of the air, this patient reſpired unmixed oxygene air for half an hour without any inconvenience! So inert was the ſyſtem! The difficulty of breathing was effectually removed, but the other complaint was not relieved.

XVII.—*Of atmoſpheres altogether artificial.*

If ever the power of elaſtic fluids on diſeaſed ſtates of the ſyſtem ſhould be inveſtigated on ſuch a ſcale as the intereſt of humanity enjoins, the trials ſhould not be limited to the addition of oxygene or unreſpirable, to atmoſpheric, air. Change the unreſpirable conſtituent part of your atmoſphere, leaving the proportions the ſame, and you will probably have an atmoſphere with different powers. Take, for inſtance, 72 parts of hydrogene, hydrocarbonate or of carbonic acid air, and add 28 parts of oxygene. What you have to expect from the ſecond of theſe mixtures, the preceding experiments will ſufficiently warn you.

Whether hepatick air, or other modifications, can ever be employed with advantage in medicine, is a ſubject for future reſearch. Mr. Watt, having lately tried various ſubſtances capable of yielding air, has juſt given me the following information, which, though it was not ſent for publication, I take the liberty to add:—" I have juſt made an air, which, as it has great powers, may, for ought I know, have great virtues; my experience extends only to its bad qualities—*Pyro-ſarcate.* I put 2 oz. of lean beef in the fire tube, and obtained, by mere heat, 250 c. i. of air, highly fœtid, like an extinguiſhed tobacco pipe; inflammable, with a very blue flame; little diminiſhed by lime and water.—*Pyr-hydro-ſarcate*, on adding water to the red-hot charcoal of this beef, I obtained 600 c. i. of air, with a foetor not ſo bad as the other; burning with an orange-coloured flame; loſing not quite $\frac{1}{13}$ in lime and water. The ſmell of the firſt made me ſick, though I did not inſpire any purpoſely, and not above one third of the quantity mentioned was let looſe in my laboratory, and 3 doors and a chimney were open; we were, however, obliged to leave the place for ſome time. The P. H. ſarcate ſeemed to poſſeſs the ſame property, but was more cautiouſly treated. G. was giddy all the afternoon. *Pyro Comate.* Next day, 2 oz. of woollen rags were put in the tube; they gave, by mere heat, 800 c. i. of air; fœtid, though not ſo offenſive as the other; burning with a deep blue flame; not tried with lime and water.—*Pyr-hydro-comate*, by addition of water

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to the red hot charcoal, gave above $1\frac{1}{2}$ cubic foot of air fœtid, but more like vol. alkali in ſmell—burning with a yellow flame; loſing 1-5th by waſhing with lime and water; part was undoubtedly alkaline air and abſorbed by the water; the water in the refrigeratory was ſtrongly impregnated with fœtid vol. alkali. Though none of either of the airs was inſpired, that could be avoided, I had a ſlight, though uncommon, nauſea, attended with ſome elevation of ſpirits, all that evening, but no heat or thirſt. In ſhort, it was very like the effect of the fumes of tobacco on an unexperienced perſon: In bed I was reſtleſs, though without pain or particular uneaſineſs, I could not ſleep. Next day the nauſea, and ſome giddineſs, continued, or rather increaſed, and a head-ache came on.—The uſes of this air, if it has any, I leave you to find out. I think I ſhall have no more to do with it, nor with animal ſubſtances: One may diſcover, by accident, the air which cauſes typhus, or ſome worſe diſorder, and ſuffer for it. J. W."

Oct. 7, 1794.

Progress of knowledge renders ſtability of language impoſſible. Hence, in treating of a new ſcience, licence of expreſſion is allowed. Whether I have abuſed this privilege by diſoxygenated *and other terms of "learned length," perſons, ſkilled alike in the philoſophy of things and words, may decide. Claſſical Scholars are commonly regarded as the beſt judges of Engliſh ſtyle; but there are circumſtances which would appear to derogate from the competency of mere Claſſical Scholars.* 1. *Progreſs in the languages of Xenophon and Tully being for ever cloſed, thoſe who have ſtudied little beſides, follow the analogy of their education, and are apt, when they regard Science, to look only backwards.* 2. *Without great precaution, their minds become frivolous, and their criticiſms will, in conſequence,*

ſequence, be captious. 3. Perſons, the moſt eminently ſkilful in Greek and Latin, now appear to little advantage in this department of Literature. HERMES *aſſigns cauſes for effects that do not exiſt; and his conceits appear like a burleſque on the philoſophy of language. Several precepts and ſtrictures, in the moſt popular introduction to Engliſh Grammar, ſeem to me miſchievous. The Author, in numerous inſtances, upon the authority of his taſte alone, or upon falſe analogies, has, if I am not miſtaken, unjuſtly condemned our fineſt writers as violators of the rules of his art. Such errors tend to diminiſh the copiouſneſs of our language, and to impair the moſt rational of amuſements.*

PART II.

PART II.

DESCRIPTION

OF AN

AIR APPARATUS;

WITH

HINTS RESPECTING THE USE AND PROPERTIES OF DIFFERENT ELASTIC FLUIDS.

BY JAMES WATT, ESQ.

Heathfield, Birmingham, July 14, 1794.

DEAR SIR,

I SEND you with this, drawings of my apparatus for producing and receiving the various airs which may be ſuppoſed to be uſeful in Medicine, with a deſcription or explanation of the apparatus, which, if you ſhall think it worthy publication, I hope may at leaſt prompt ſome younger and more active man to contrive a better.

In conſequence of your deſire, Boulton and Watt have agreed to manufacture theſe machines for the Public. You wiſh that a price could be fixed, but that cannot be done at preſent, until ſome more are made, and our workmen underſtand it. At preſent, they are but bunglers in executing ſome of the parts, and improvements may ſtill be made, which may encreaſe or diminiſh the price.

However, we have no deſire to be the manufacturers, except to ſupply thoſe who may not have the ſame opportunities as ourſelves of procuring them; the price ſhall be as moderate as we can make it; and thoſe who chooſe to have them made by others, ſee what is to be done.

In a former letter, I mentioned my ideas upon the airs which I apprehended might be uſeful in conſumptions, and ſome other diſorders of the lungs; but as I then obſerved, it is not my province to preſcribe; that muſt be left to the phyſician. It may be right, however, to ſay, that, according to the experience of ſeveral perſons here, the inflammable air from charcoal, on being inhaled into the lungs, renders the perſon who breathes exceſſively giddy and ſick, in a very ſhort time; from whence I conclude, that ſome new compoſition or abſorption takes place in the lungs; probably the charcoal which it contains in ſolution is abſorbed by the blood; and if ſo, it may prove an antidote to the oxygene the blood is ſuppoſed to contain, in too large quantities, in conſumptive patients. I ſhould think, that on account of its great activity, this air ought to be given in a very diluted ſtate, otherwiſe it might prove too much for the patients.

The inflammable air from zinc contains the calx or flowers of that metal, in a ſtate of ſuſpenſion, if not of ſolution, and may alſo be uſeful in curing ulcers in the lungs, as a topical application. This air does not render the patient ſick; but in an experiment I made on myſelf, it ſeemed very much to thicken the mucous matter ſpit up in the morning after.

Again wiſhing you to be ſucceſsful in this undertaking, which promiſes to be of ſo much utility to mankind, I remain, with much eſteem,

DEAR SIR,

Your obedient humble ſervant,

James Watt.

To Dr. BEDDOES.

P. S. If any ſets of apparatus are wanted, pleaſe addreſs to Boulton and Watt, Birmingham.

DESCRIPTION

Description *of an* Apparatus *for procuring various kinds of* Air, *for* Medicinal *purpofes.*

THE apparatus confifts of an alembic, or pot (A) and its capital (B). The latter is connected by a pipe (F) with the refrigeratory or wafhing veffel (G), which again communicates by a pipe with the Hydraulic Bellows (H. J.) which receives the air as it is generated or produced, and transfers it into oiled filk bags, or other veffels, from which it may be conveniently inhaled by the patient.

The pot (A) is made of foft caft iron, of the form in the drawing, and is about fix inches diameter in its wideft part or *bilge*; the thicknefs of the metal about half an inch. The lower part of the capital (B) is made conical, and ground into the mouth of the pot, fo that it may be made tight with a fmall quantity of cement. An iron tube, half an inch infide diameter, paffes perpendicularly through the centre of the capital, and reaches within half an inch of the bottom of the pot. This tube is continued four or five inches above the capital, where it ends with a conical mouth at (C). It is fitted into the capital by a conical fwelling, which is ground into a correfpondent hole, and made tight with cement. Another tube (D C), made conical at its lower end, is fitted into the conical mouth (C) of the lower tube. The lower end of the tube (D C) is contracted fo as to form a hole of about 1-20th inch diameter, which is capable of being ftopt or fhut by the end of the wire (E), which is fitted to it. This tube is one foot long, and has a cup or bafon at its upper end. The wire (E) is formed into a fine threaded fcrew at the upper end, and is fcrewed through a bridge in the cup, fo that by turning the wire, it may open or fhut the hole at the bottom of the tube, as occafion requires. The proper taper, for thefe feveral cones, is about one in eight (that is, for every inch they are long, they fhould be 1-8th of an inch lefs in diameter at the fmaller than at the larger end).

The tube (F), which conveys the air from the pot to the refrigeratory, is about 1-$\frac{1}{4}$th inch diameter, and taper for about three inches at both ends, which enter into the pipe ſide of the capital and the receiving pipe (N) of the refrigeratory. This tube is made of ſheet iron brazed with hard ſolder. The length of this tube is from three feet to ſix feet, as ſuits the convenience of the operator.

The refrigeratory (G, S S, Pl. 2. Fig. 2 and 3) conſiſts of two veſſels, of which one is placed in an inverted poſition, within the other. The outer veſſel is cylindrical, about twelve inches diameter, ſeven inches deep, and open at top. On one ſide there is a funnel (R. ſee Pl. 2. Fig. 2.) and a ſemicircular pipe attached to the outſide of the veſſel, paſſing through the ſide of it near the bottom, and continued inwards along the bottom for about 1$\frac{1}{2}$ inch. On the oppoſite ſide of the veſſel are two ſhort pipes, about an inch diameter each, the upper one for letting off hot water, and the lower one for emptying the veſſel when required.

The inner veſſel of the refrigeratory (S S Pl. 2. Fig. 3.) is alſo cylindrical (of ſuch diameter as to enter eaſily into the outer veſſel), and is open at bottom. The cover of it is convex upwards, about an inch. Upon the underſide of this cover is formed a ſpiral channel, by means of metalline plates, about 1$\frac{1}{2}$ inch deep, and one inch aſunder. This ſpiral channel is open below, but the edge of the ſlip of metal which forms it is well ſoldered to the concave ſide of the cover. The receiving pipe enters this channel cloſe to the circumference of the veſſel S S, and the diſcharging pipe O P is fixed upon the central end of the ſpiral. Cloſe to the pipe O, is another ſhort pipe, which paſſes through the cover of S S, and reaches downwards about an inch and a half, being open at both ends. In the edge of the veſſel S S is a notch (T), which, when that veſſel is placed within the veſſel G, receives the lower end of the pipe R, and permits the rim of the veſſel S S to reſt upon the bottom of G; in which poſition it is to be retained, by laying weights upon it.

The diſcharging pipe conveys the air to the Hydraulic Bellows (H J), which conſiſts of two veſſels (H and J, Pl. 2. Fig. 1.) The outer veſſel H conſiſts of two

cylinders,

cylinders, placed one within the other, and about half an inch asunder. These cylinders are joined together at bottom by a circular rim, well soldered to them both; and the inner cylinder is shut at top by a cover also soldered on. This inner cylinder is about two inches shorter than the outer cylinder, and the latter is surmounted by a cup (W W) about $1\frac{1}{2}$ inch deep, and one inch all round, more diameter than the cylinder it is attached to. The pipe P Q passes diametrically across the vessel H; the end Q is open, and made so as to be stopt with a cork or cock. From this pipe P Q proceeds a pipe (V), which passes upwards through the cover of the inner cylinder, to which it is soldered, and is open at its upper end. The second vessel J of the bellows is a hollow cylinder of one foot diameter, and eighteen inches long, shut at top, and open at bottom; it is made so as to move up and down easily in the circular interstice between the inner and outer cylinder of the vessel H; and when that interstice is filled with water, as high as the cover of the inner cylinder, if the vessel I is moved up and down, it will act the part of a bellows, drawing in and blowing out air, by the pipes V and P Q. The bellows and refrigeratory are made of tinned iron plates japanned, or of tinned copper-plates not japanned.

Fig. 4, Pl. 2, is a section of the pot, its capital, and the pipe which passes through the latter.

Fig. 5 is a section, of the natural size, of the upper and lower ends of the wire E, and of the lower end of the pipe D C, which admits water into the pot.

Fig. 2, Pl. 1, is a plan of a large chafing dish or furnace, in which the pot may be placed. This furnace, and its ash-hole, may be made of black sheet iron, and the furnace lined with fire lute or Windsor bricks. The cover is made in three moveable segments of a cone fitted to the mouth of the furnace, and to the neck of the pot, but leaving an opening on the side opposite to the pipe F, for the issue of the smoke and flame. These segments should also be lined with fire lute.

Fig. 3d is a section of an iron tube, which may be used in place of the pot, if found preferable.

MANNER

Manner of using the Apparatus.

The pot is to be filled with a ſufficient quantity of the material, by means of which, or from which, the deſired air is to be procured. The lower end of the capital is to be anointed with a ſmall quantity of the proper cement, put into the mouth of the pot, and turned round a little, preſſing it down at the ſame time, until the joint is concluded to be made good. The pot is then to be placed in the furnace upon its pedeſtal (of iron or brick), and a fire of good coaks is to be made round it. The covers of the furnace being put on, the pipe C is to be anointed with cement on its lower cone, and twiſted forcibly into its place in the capital. The joints of the pipe F are to be cemented at both ends, and the two veſſels of the refrigeratory being previouſly put one within the other, the two ſhort pipes in the ſide are to be ſhut with corks or cocks, and the veſſel G is to be nearly filled with cold water; the joint of the pipe P, with the bellows, is to be cemented, and the outer veſſel H of the bellows filled with water to the height indicated (the veſſel J being placed within it, and preſſed down to the bottom), and the pipes O and Q are to be ſhut with corks.

In the proceſs for producing inflammable air from charcoal, iron, or zinc, when the pot is become quite red hot, as high as the neck, the wire E being ſcrewed down ſo as to ſtop the opening, the pipe D C is to be put in its place, and the baſon D filled with water, then, upon unſcrewing the wire E about a turn, ſome of the water will drop into the pot, and the inflammable air will immediately be produced, and will proceed by the pipe F to the refrigeratory, where it will diſplace ſome of the water, and glide along the ſpiral channel to the centre, depoſiting its heat, or other matters capable of uniting with water, as it paſſes in contact with it. It will then paſs quite cool by the pipe O P into the bellows. The weight L (which is made juſt to counterbalance in air the weight of the veſſel J) will, by means of the cord attached to J and paſſing over the pullies K K, pull up the veſſel J in proportion as the air enters.—When the veſſel J is drawn up till its cover touches the frame M M, the cork Q is to be taken out, and the wooden nozzle of an oiled ſilk

bag,

bag, hereafter defcribed, is to be forced into the hole, fo as to be air tight. The veffel J being then preffed down by hand, the air will pafs into the filk bag; when the bellows is emptied, prefs your thumb on the outfide of the filk againft the outer orifice of the wooden nozzle, withdraw the nozzle from the pipe Q, and replace the cork, that the operation may go on as before.

The air in the bag may then have a proper proportion of atmofpheric air added to it; and when thoroughly mixed, can be inhaled with great eafe from the nozzle of the bag, efpecially if the latter is laid on its fide on a table.—When the water in the refrigeratory becomes hot, the cork of the upper fhort pipe in its fide is to be taken out, and cold water poured down the funnel R, which, being fpecifically heavier than the hot water, will difplace it, and the latter will iffue upwards through the fhort pipe near the centre, and pafs off by the pipe at the circumference. When the water feems fufficiently cooled, the cork is to be replaced, and the operation will proceed as before: indeed it need not be ftopt while the water is thus changed.

In producing air by means of charcoal, the charcoal fhould be previoufly burned till it yields no fmoke; and care fhould be taken that nothing capable of doing fo enter the pot. There fhould be as much charcoal put in as will fill the pot up to the neck or cylindric part; it fhould not be pounded, but broken into little bits, about the fize of a hazle nut; but what is inevitably made fmaller in the breaking, need not be rejected; it will be proper to force an iron down through the charcoal, to open a way for the pipe, before the latter is put in.

When the air is wanted to be made by means of iron, the beft is the turnings of *hammered* iron, to be had from the jobbing fmiths. Caft iron turnings or borings always give fome very ftinking hepatick air.

If the air is to be obtained by means of zinc, the metal fhould be granulated, by pouring it, while melted, in a fmall ftream into water; and the water pipe of the capital fhould either be cut fhorter, or fome fmall holes drilled in its fide, as the water could not eafily force its way through a pillar of the melted metal; only a few pounds of this metal fhould be put in at a time, as only

the furface can be acted upon, and that is foon covered with the melted calx. As zinc has a great affinity with iron, and the heat required in this cafe is confiderable, it will be proper to coat the infide of the pot with a thin covering of China clay, or fome other apyrous earth, mixed up with a folution of borax, to prevent the contact of the metals.

In obtaining dephlogifticated or oxygene air from manganefe by heat, no water muft be added, and the hole for the water pipe fhould be ftopt by an iron plug fitted to it. But probably an iron veffel, made in the form of a Florence flafk, may be a more commodious veffel for this purpofe, which remains to be tried. In any cafe, the refrigeratory and bellows remain the fame. I can give no *particular* directions in the procefs for obtaining dephlogifticated air, in this way, having never practifed it.

It appears probable that the fire tube, Fig. 3, Pl. 1, may be found a more commodious or effective veffel than the pot, but I cannot yet determine that point, not having hitherto had time to get it tried. Both Mr. Lavoifier and Dr. Prieftley ufed tubes. I have added the new water pipe, which admits the water by means of a conical wire, inftead of a cock, ufed by the former, or of the retort ufed by the latter, and have made the operation of fhutting the ends eafier.

The pot of the fize indicated will produce about a cubic foot of inflammable air from charcoal in five minutes, with a good fire.

The quantity of water to be admitted into the pot muft be regulated by experience, if the pipe at N becomes very hot, and a loud cracking noife is heard in the refrigeratory, too much water is thrown in, and fteam reaches the refrigeratory. When air only iffues from the pot, the pipe at M will not be much heated.

The joints at N and P may be cemented with a dough, made of the Cornifh china clay with fome flour and water, or with the common chemical lute, compofed of whiting, falt, flour, and water. The joints of the capital with the pot, and with the pipes F and C, muft be made good, by a cement of the Cornifh china clay, moiftened with a faturated folution of borax, or by a mixture of fine flaked lime and folution of borax, which latter will, I apprehend,

apprehend, be improved by the addition of colcothar of vitriol. Care muſt be taken not to ſhake the veſſels, after the joints are made, leſt the cement ſhould crack, and let out the air.

The oiled ſilk bags ſhould be made in the form of a common ſack, and have a wooden nozzle fitted to them in the ſhape of a common faucet, with the ſmaller end outwards, that it may fit into the tube of the bellows; and this faucet ſhould be provided with a ſpiggot to keep in the air when required. Oiled ſilk has commonly a very diſagreeable ſmell, which, as it is not certain that it would be of ſervice in any of the diſeaſes that may be treated by theſe airs, it is deſirable to get rid of; this I have accompliſhed by the following proceſs:

Take charcoal, freſh burnt till it is free from ſmoke, reduce it to powder, and ſift it: Lay the ſilk on a table, and ſift this charcoal over it, to the thickneſs of a quarter of an inch; then roll the ſilk, charcoal duſt, and all, upon a roller, as long as the ſilk is wide, and not leſs than an inch and a half in diameter; bury the whole in charcoal duſt, and let it lie two or three days; unroll it, and ſweep off the charcoal; and if the ſmell is not quite gone, repeat the operation with freſh charcoal duſt; when free from ſmell, waſh it clean with a wet ſponge. When the ſilk is made into a bag, anoint the ſeams with japanners' gold ſize, diluted with ſome oil of turpentine, which will dry ſufficiently in a few days.

In chuſing oiled ſilk for this purpoſe, that which is green ſhould be avoided, as it is coloured with verdegris, which adds to the bad ſmell, and rots the ſilk. The yellow, or yellowiſh, is the beſt.

As patients, when very weak, cannot be made to breath out of a bag or other veſſel through a mouthpiece, I think the inflammable air may be adminiſtered by placing upon their head a cap in the form of a beehive, about a foot diameter at the baſe, reaching as far down as their chin. If the pipe of the bag, containing the air, is placed near their mouth, and the bag gently preſſed, as inflammable air is lighter than atmoſpheric, the cap will be filled with it, and they muſt infallibly

breath it, and at the ſame time will be under no teazing conſtraint.

SUPPLEMENT TO THE DESCRIPTION AND USE OF THE APPARATUS.

EXPERIENCE has ſhewn, that for private practice, an apparatus on a ſmaller ſcale than that deſcribed will be ſufficient; and one is now conſtructing of only a quarter of the contents, the bellows of which will be about nine inches diameter, and thirteen inches high, and the other parts proportionable. The furnace, twelve inches outſide diameter, will have a fire tube two inches inſide diameter, and ſixteen inches long, nine inches of which being expoſed to the fire, will, it is expected, produce air in ſufficient quantities.

As it appears from experiments made by Doctor Beddoes, that oxygene air is produced moſt eaſily, and in the greateſt purity, from Exeter manganeſe and oil of vitriol, by diſtillation in a glaſs retort, a ſand pot is fitted to the mouth of the furnace, a ſhort bent chimney pipe is adapted to one of the holes made for the fire tube, and a fire door at which coaks may be put in, is fitted to the other, ſo that the furnace may be uſed for that or any other chemical diſtillation; and to regulate the fire, an air hole, with a ſliding damper plate, is made in the ſide of the aſhpit.

On all occaſions, when this or the larger furnace is uſed, it will be proper to ſhut the door of the aſhpit, and to admit the neceſſary air by the air hole, otherwiſe, when the coaks are good, the fire may become too hot, and injure the pot or fire tube; the proper heat for which is a moderate red heat, or the loweſt heat, at which experience ſhows that the deſired air is produced.

For infirmaries, apothecaries or others, who wiſh to make large quantities of air, the larger apparatus may be uſeful; but as from fifty to one hundred cubic inches of the heavy inflammable air, or hydrocarbonate, mixed with the proper quantity of common air, is a ſufficient doſe, or perhaps three or four times that quantity of the pure inflammable

inflammable, or hydrogene air, if the apparatus can make three or four cubic feet in an hour, it would ſeem large enough. Where fixed air is wanted to be made from chalk or marble, by means of water and heat, or from manganeſe by mere heat, the larger apparatus may be neceſſary, as the ſmaller would not contain enough of the materials.

The fire tubes, upon trial, ſeem to anſwer perfectly well, and in many caſes are preferable to the pots, tho' in other caſes, the latter are more convenient. The ſmaller apparatus will not admit of the uſe of a pot; but thoſe who need a large apparatus, may have the furnace adapted for both, as well as for a ſand heat.

The pot anſwers very well for diſtilling empyreumatic oils, or other ſubſtances of that nature; but in this caſe, a common glaſs receiver ſhould be adapted to the conducting tube in place of the refrigeratory.

As it is troubleſome to empty the hydraulic bellows of the artificial air every time we want to meaſure common air into the oiled ſilk bags, it will be found convenient to have an additional bellows for that uſe, and the ſmaller ſize will be in general ſufficient. When you have introduced into the bag the quantity of artificial air you intend, fill the ſpare bellows with common air, apply the faucet of the bag to one of the pipes, and ſhut the other with the hand, or otherwiſe; then diſcharge the contents of the bellows into the bag, ſtop the faucet, by applying your finger on the outſide of the bag, to its orifice; then admit air into the bellows, by opening the other pipe, and proceed as before, until you have put in the deſired quantity.

This operation may be facilitated, by fixing a cock into the pipe, by which the bellows receives the common air.

As every patient may not have conveniencies for fixing an apparatus, and the bags cannot be depended upon for containing the air for any length of time, it is deſirable to have ſome means of ſending theſe artificial airs to a diſtance, or for ſtoring them up to avoid the neceſſity of heating the furnace on every occaſion. The following has occurred as a means of anſwering both theſe ends. Let a cylindrical veſſel be made of ſtrong tin plate

plate (as in Fig. 1, of Pl. 3.) this veſſel is to be cloſe at both ends, which are made concave outwards, cloſe to both the bottom and cover, ſhort pipes U and V proceed from the ſide of the veſſel, their diameters ſhould be the ſame as the pipe P Q of the bellows.--- Another pipe (T) paſſes through the middle of the cover or upper end of the veſſel, to which it is well ſoldered, and reaches within half an inch of the bottom. To guard this veſſel from ruſt, it ſhould be japanned both inſide and out; and for the greater convenience of japanning it within, it may be made to come aſunder at the middle of its height; and when varniſhed, may be cemented together, by a mixture of wax and roſin uſed hot.

When this veſſel is completed, the upper pipe (U) is to have a ſhort pipe (W) inſerted into it, which ſhould alſo fit the pipe Q of the bellows. The lower pipe (V) is then to be corked, and the veſſel filled with water, by the central pipe (T). This veſſel, which may be denominated an *air-holder*, is to be placed in an empty tub, the pipe W inſerted into the pipe Q of the bellows, and cemented to it.

When the bellows are filled with artificial air, the cork of the lower pipe (V) is to be taken out, and the counterpoiſe of the bellows is to be lifted up; the water in the *air holder* will then run out into the tub, and the air deſcend from the bellows into the former veſſel. The air holder, thus filled with the artificial air, and all its pipes cloſely corked, muſt be kept in a cool place till wanted. To transfer the air from this veſſel into a bag, fix the faucet or mouth-piece of the bag into the upper pipe (U), and if you want a quart, or gallon, or other meaſure of air, pour ſo much water into the air holder through the central pipe, and exactly that quantity of air will iſſue out into the bag; then re-cork your veſſel, until you want more air from it. When the air holder is larger than the bellows, it will be proper to have a cork fixed in the lower pipe (U), at which the water may be ſuffered to iſſue no faſter than air is produced by the apparatus, otherwiſe the pillar of water in the air holder will draw in common air, through the openings of the refrigeratory.

If

If ſeveral of theſe veſſels are provided, a ſtock of air may be laid up for as many days as experience ſhall ſhew that the air will keep good, and doſes may be eaſily ſent to diſtant patients. The ſize of the veſſel muſt be regulated by convenience, but may anſwer very well if the veſſel is made a cylinder of twelve inches diameter, and ſixteen inches high, which is about equal to the contents of the bellows of the larger apparatus.

Fig. 2, Pl. 3. ſhews the form of a chafing diſh for collecting a mixture of azotic and fixed airs from burning charcoal, and a refrigeratory for cooling it, as it paſſes to the bellows.

The chafing diſh (A) is to be filled *quite full* with burning charcoal. The trough of the refrigeratory (B) is to be filled with water, and the end (C) of the pipe is to be joined to the pipe (P) of the bellows; when the charcoal has ceaſed to ſmoke, the bellows are to be raiſed very ſlowly; the air which has ſerved to animate the fire will paſs into it; and when the proceſs is judiciouſly performed, will be found to contain no uncombined dephlogiſticated air.

Fig. 3. Pl. 3. is a ſection of a circular refrigeratory for the ſame purpoſe, the upper part of the veſſel is to be kept ſupplied with cold water, which will deprive the air of its heat as it paſſes through the veſſel, but no water is to be put into the ſpace below the diaphragm. The air is admitted by the pipe (a), and iſſues by the pipe (b) to the bellows.

When fixed air is to be produced from red hot chalk or marble, by means of water, this latter refrigeratory ſhould be uſed, as the circulating refrigeratory deſcribed in Pl. 1 and 2, by bringing the air in contact with the water, much of it would be abſorbed, and would never reach the bellows. The ſpace below the diaphragm ſerves to collect the water proceeding from the condenſed ſteam, ſome of which will always come in with the air.

Fig. 4, Pl. 3, is a ſection of the furnace for the ſmaller apparatus, furniſhed with a ſand pot, to be uſed as a diſtilling furnace. When it is uſed with the fire tube for producing inflammable air, &c. the chimney pipe (a) and pot (b) are taken off, and the tube is introduced through the openings left for the fire door and chimney; and as the

the fire tube is made a little ſmaller at the ends than in the middle, the vacuities round it in the holes of the ſides of the furnace are filled up by two rings of caſt iron, fitted to them, and to the ends of the fire tube; and in this caſe, when a fire is made, the furnace is to be filled up to the mouth with coaks.

The outſide diameter of this furnace is twelve inches, and the inſide diameter within the brick lining is nine inches; the height from the grate to the top is nine inches; and the depth of the aſh pit below the grate is ſeven inches. Windſor bricks, rubbed to the form, make a very good lining.

In producing theſe airs, if the quantity of water admitted through the water pipe is no more than can enter into the compoſition of the air, the latter ſeems to be more exalted in its properties than when more water is admitted; and it alſo ſeems to carry with it, in a ſuſpended ſtate, more of a powdery matter, which appears to be part of the ſolid ſubſtance from which the air was prepared; this powder is depoſited when the air ſtands ſome time quieſcent; and therefore it would ſeem that there ſhould be a difference between the medicinal virtues of the air, when freſh made, and thoſe it poſſeſſes after ſtanding ſome time, which requires the attention and obſervation of practitioners to regulate the doſes in its ſeveral ſtates, to determine in which of them it ſhould be ſerved; or whether it ſhould be uſed at all before it has ſubſided.

The proceſs for making the inflammable airs ſhould not be carried on by candle light, if it can be avoided; as the ſtream of air, when diſcharged from the bellows, would catch fire by the approach of the candle, and produce dangerous exploſions.

The firſt produce of the air, in any of the operations, ſhould be rejected, as its purity will be uncertain.

The uſers of the apparatus will do well to ſtudy the means of judging of the purity of the airs they employ; for which purpoſe, an attentive peruſal of Doctor Prieſtley's Treatiſes on Air is recommended.

Birmingham,

Birmingham, June 17, 1794.

Dear Sir,

HAVING never made the art of medicine my particular ſtudy, I ſhould not have troubled you with my crude ideas upon the uſe of pneumatic medicines, if your approbation of what I mentioned to you, joined to my earneſt deſire to aid your endeavours, with the hope that poſſibly ſome idea might be ſtarted, which may ſave other parents from the ſorrow that has unfortunately fallen to my lot, had not urged me to ſtep over the bounds of my profeſſion.

It appears to me, that if it be allowed that poiſons can be carried into the ſyſtem by the lungs, remedies may be thrown in by the ſame channel. Remedies for ſome fatal or dangerous diſorders may, poſſibly at leaſt, be found in the claſs of airs which admit of many known modifications, and doubtleſs many more ſtill to be diſcovered :—which of theſe may prove beneficial in conſumption, and other analogous diſorders of the lungs, remains to be aſcertained by experiment. You have ſhewn that oxygene air is hurtful in many of theſe diſorders, though beneficial in ſome caſes of aſthma ; its oppoſites inflammable, azotic, and fixed air, ſeem then to be thoſe which are moſt likely to be uſeful in phthiſis : But there are alſo ſubſtances which ſome eminent phyſicians have thought might be uſefully employed even in the ſtate of powder, ſuch as Peruvian bark, the calces of lead and zinc, with other aſtringents.

To the uſe of powders, however finely *mechanically* divided, I think there are ſome objections ; particularly I doubt whether they could enter the minute veſicles of the lungs ; but if ſuch ſubſtances can be *chemically* divided and obtained in the ſtate of ſolution in air of ſome congenial ſpecies, they might have their full effect.

It is well known, that inflammable air, when produced by the common proceſs from iron and vitriolic acid, always carries with it, even through water, a large quantity of iron ; ſome of which it afterwards depoſits, but very probably ſome part ſtill remains ſuſpended. If iron ſhould then be eſteemed a proper medicine for diſorders of

of the lungs, we are thus furnifhed with the means of obtaining it in a sufficiently divided ftate; and to free it from any adherent acid, it may be paffed through a cauftic alcali.

If the calx of zinc is thought preferable, it is fufpended in inflammable air in great quantities, by applying water or fteam to red hot zinc in clofe veffels, and probably alfo by the common procefs of making common inflammable air from zinc by vitriolic acid. The calces of zinc are very efficacious in healing external fores; and are very likely to be fo in internal ones, provided they can be applied, as I think they may, by the means indicated.

Charcoal has lately been found extremely efficacious in correcting putridity, and in difpofing ulcers to heal. It feems to me, that no fubftance is diffolved in common inflammable air in fuch quantities as charcoal, nor more intimately united. If water is applied to red-hot charcoal in clofe veffels, the heavy inflammable air is produced in large quantities; and this air has been found to contain inflammable air, properly fo called, fixed air, feparable by water or by alkalies, and fome other fubftance, which, when the inflammable air is deflagrated with oxygene air, produces fixed air. This fubftance I confider as charcoal in a ftate of folution; for were it fixed air completely formed, it would be feparated by the means mentioned. Whether charcoal in this ftate could be decompofed by any excefs of oxygene in the blood of confumptive patients, I cannot fay; but it feems likely that it would; and at any rate it would act as charcoal powder does, and therefore highly merits trial.

As fixed air is a faturated folution of charcoal in oxygene air, it is not probable that the lungs can decompofe it; we fhould therefore only look to its effects as an antifeptic. As the lungs, when doing their duty, fhould feparate, and throw out fixed air, it is not probable they will abforb it, though it may have fome effect merely by excluding the oxygene of the common air. I think, however, it will be found to have moft beneficial effects in cafes of a putrefcent tendency; or if you do not like this theoretical phrafe, where the breath and expectorated matter are fetid. The fpecies I would recommend

is

is that from fermentation and the means, keeping a veſſel of fermenting wort cloſe by the patient, which will in general be found grateful to him.* Fixed air, from vitriolic acid and calcareous earths, may be occaſionally much contaminated by other acids. The oil of vitriol of commerce is generally impure, containing ſulphureous acid, with the nitrous and marine; it ſhould be rectified for the purpoſe of medicine.

Inflammable air is ſaid to act as a ſolvent of dead animal ſubſtances; it may therefore act favourably upon the viſcid mucus, ſometimes ſo troubleſome in conſumption.

I can conceive no action of the azotic air, except the negative one, of excluding the oxygene.

If aleppo galls are ſubjected to a heat a little greater than that of boiling water, they throw out much fixed air united with their own peculiar acid, and a very aſtringent taſte and ſmell. The acid may be moſtly ſeparated, by paſſing the air through water. You can judge better than I, whether this aſtringent air might not be uſeful in ſome caſes.

The hepatick inflammable air may have good effects in other caſes; but its ſmell is very diſagreeable to the patients; it may be obtained from alkaline or calcarious hepars, or from ſulphurated metals, by means of vitriolic acid.

The inflammable air of marſhes is ſuppoſed to cauſe fever, which is ſaid to be a diſeaſe of an oppoſite nature to pthiſis, at leaſt to ſuſpend it. If I remember right, this air is ſimilar to that obtained from red-hot charcoal by water, but with the addition of hepatick air.

If it be certain that butchers are exempt from phthiſis, putrid animal effluvia may be uſeful; and if the matter which conſtitutes the ſmell be not the uſeful part, it may be corrected by powder of charcoal, which does not otherwiſe hinder the progreſs of putrefaction. The ſmell ſeems to be owing to ammoniacal hepatic air.

The mixture of azotic and fixed air to be obtained from burning charcoal (firſt freed from bitumen by heat) might be tried, but I ſhould hope more from the heavy inflammable air of charcoal.

* I know that Mr. W. ſpeaks here from attentive obſervation.—T. B.

This latter ſpecies is recorded by Dr. Prieſtley to have been produced moſt rapidly, from a mixture of ſcales of iron and charcoal, ſubjected to a ſtrong heat in an earthen retort, and in this caſe would probably contain much iron.

The oxygene air may alſo be impregnated with various ſubſtances. When it is made by paſſing the ſteams of ſp: nitri through a red hot tobacco pipe, it is highly charged with a white powder, ſome part of which it lays down on the contact of water; when produced in glaſs veſſels, I have never ſeen it contain any ſuch white matter. An eminent phyſician, of your acquaintance, previous to my mentioning to him the ideas I now ſend you, obſerved to me, that the oxygene air from heated manganeſe, had a peculiar taſte and ſmell; and that unleſs ſome other facts led to aſcertain the ſubject, he ſhould be at a loſs to determine whether ſome of the cures you mention *might* not be attributed as much to the manganeſe as to the oxygene. He alſo, a priori, had entertained ideas of the good effects of ſubſtances diſſolved in airs.

Upon my preſent degree of knowledge, were I to try to diſſolve metallic or earthy ſubſtances in oxygene air, I would place the impregnating earth or calx in a well coated glaſs tube, heated red hot, and paſs the ſteam of ſpirit of nitre through it, afterwards waſhing the air with an alkali.

You will now, my dear Sir, think that I have gone far enough with mere hints, ſupported only by analogy, and may perhaps have perceived, that however incompetent, I have ventured to differ a little from you. I am for conjoining the operation of ſubſtances likely in themſelves to prove ſanative, to the privation of oxygene, which, however, I do not diſpute may of itſelf produce good effects.

In regard to the manner of breathing theſe medicinal airs, I think it will be done beſt from bags of ſome very flexible and light ſubſtance, ſuch as very thin leather waxed, or oiled ſilk. If a ſmall tube be inſerted into the mouth of the bag, the air may be preſſed out oppoſite the patient's mouth, in caſes when they are too weak to make extraordinary exertions of the lungs, or rooms may be filled with the proper mixture of airs.

It

It would be desirable that a list were made out of all substances, which are known to be soluble in air of any kind, or are of themselves reducible to vapour or steam, that experiments may be made upon their sanative effects in cases of diseased lungs. The list will prove more numerous than may appear at first glance.

Having now explained my general ideas, I submit them to your correction,

And remain, with much esteem,

Your's,

James Watt.

To Dr. Beddoes.

Birmingham, Sept. 2, 1794.

Dear Sir,

YOU desire me to send you a more particular account of my observations on the medicinal airs than was contained in my former correspondence on that subject. In my letter of June 17th, I mentioned that it seemed to me that the heavy inflammable air, or carbonated hydrogene, being principally a solution of charcoal in inflammable air, was more likely than any other to correct any disease arising from super-oxygenation of the blood. I could not, however, foresee that its effects would be so powerful in some respects as they have proved. In the beginning of July, I made some of this air by the application of water to red hot charcoal in a close vessel. Its smell was somewhat hepatick, from the new cast iron vessel it was made in, and was also contaminated, by a bad lintseed oil varnish in the refrigeratory, its taste was that of fixed air, though more feeble. I inhaled a little of it cautiously, but had scarcely withdrawn the pipe from my mouth before I became so giddy, that I could not stand without a support. I had also considerable nausea. A healthy young man, who stood about 6 feet from the hydraulic bellows when I discharged about a cubic foot of this air, was affected in the same manner, as it passed by him towards an open door. Another young person, merely from smelling to it as it issued from the bellows,

 fell

fell upon the floor infenfible, and wondered where he was when he awaked. None of us experienced any difagreeable effects in confequence of the vertigo, &c. only in going to bed 6 hours afterwards, I felt fome fmall remains of the vertigo. Several other perfons have inhaled it fince; and all were affected in the fame manner. I have no doubt, from what I have obferved, that if inhaled in a pure ftate, this air would fpeedily bring on fainting and death; when given as a medicine, it ought therefore to be much diluted with common air, I fhould think, with 12 times its own bulk. Its effects upon difeafed lungs you are better qualified to fpeak to, and truft you will give the neceffary cautions for the ufe of fo active a medicine, in a more diftinct manner than I am qualified to do.

About the fame time, I made fome inflammable air by means of zinc; it contained a very confiderable quantity of the flowers of that metal in a ftate of fufpenfion, which had the appearance of grey fmoke, as it was difcharged from the bellows. I breathed this air 3 or 4 times without being fenfible of any immediate effect; nor could I have diftinguifhed it in that manner from common air, though when I blew it out of my lungs againft a lighted paper match, it took fire. Next morning I fpit up fome mucus very folid, and at moft as elaftick as caoutchouc, and the fame in a fmaller degree the fecond morning; this I attributed to the calx of zinc, which I apprehend it contains in a ftate of folution, as well as of fufpenfion.

Of fixed air, I have little to fay. I have occafionally breathed it in larger quantities than were agreeable, and always experienced flying ftitches in the mufcles of my breaft in confequence, but they foon left me without any medicinal help.

Confidering that no fpecies of artificial air is obtained except water is obvioufly prefent, or that there is reafon to fufpect it may be contained as an element, or part of one of the fubftances concerned, and that Dr. Prieftley obtained fixed air from aerated barytes, by paffing fteam over it when in a red hot ftate, though it would yield none by a mere dry heat, I concluded, that if water or fteam were applied to calcareous earths when redhot, they would readily part with their fixed air. I put 1½lb. of chalk

chalk broken into ſmall pieces into the pot of my apparatus, and, when red hot, admitted ſmall quantities of water. I obtained about 4 cubic feet of fixed air, extremely pungent to the ſmell, and greedily abſorbed by water. The laſt portion was mixed with ſome inflammable air from the iron pot, and the chalk was found to be nearly cauſtic, but had no way changed its form.

This air was free from any ſmell ſimilar to that of aquafortis, which that produced by means of vitriolic acid generally has, and perhaps was more pure.

In purſuance of the ſame idea, I concluded that nitre might yield its dephlogiſticated air leſs reluctantly, if water were added when it was redhot. I put 4 ounces of nitre into an iron pot, and, by mere heat, obtained about 400 cubic inches of air, which, being waſhed in its paſſage through the ſpiral refrigeratory, did not taſte of ſpirit of nitre, though it ſmelled ſlightly of it. Fearing that on the addition of water ſome inflammable air might be produced, and there might be an exploſion, I removed the refrigeratory and bellows, and then admitted ſome water. Air immediately iſſued in quantities from the conducting pipe of the pot; and this air was found, on the application of a match, to be dephlogiſticated; but ſome ſpirit of nitre iſſued at the ſame time, and probably ſome azotic air. The pot was conſiderably corroded by the nitre, which had found an iſſue at ſome defective places, that has hitherto prevented a more complete experiment from being made. It would ſeem, from theſe appearances, that my reaſoning was right, and that nitre may in this way be made to yield all its air in a moderate heat. It ſtill, however, remains a deſideratum to find veſſels which can retain in it a red heat for a ſufficient time.

I put $1\frac{1}{2}$ pound of the Mendip manganeſe you were ſo kind as to ſend me, into the iron pot, and, by dry heat, obtained from it about $1\frac{1}{2}$ cubic foot of air; the firſt and laſt portions ſeemed, by the taſte, and by its extinguiſhing flame, to be fixed air, about half a cubic foot was dephlogiſticated. When it it had ceaſed to give air by that heat, I added water, and obtained a conſiderable quantity of fixed air, ſimilar to that from chalk, but in which a grey powder was ſuſpended in conſiderable quantities,

quantities, which gave the appearance of ſmoke, as it is iſſued from the bellows. A perſon who breathed a little of this air undiluted, experienced a ſlight vertigo and nauſea. May not this proceed from the powder ſuſpended in it?

The purity of the dephlogiſticated air, which you obtained by means of vitriolic acid from the Exeter manganeſe, may not be wholly owing to its ſuperior purity, but to your mode of diſengaging it; for I apprehend concentrated vitriolic acid will diſengage very little fixed air, even from marble, as it ſoon covers it with a coat of gypſum, which protects it from any further action of the acid. If, therefore, this air can be freed ſufficiently from any taint of the acid, the method you have followed ſeems by much the beſt mode of obtaining it, and perhaps the cheapeſt.

In reſpect to pure azotic air, I have tryed no proceſſes, but the method I mentioned to you in June laſt, of obtaining a mixture of azotic and fixed air from burning charcoal ſucceeded perfectly,

I made a chaffing diſh about 6 inches diameter, and nine inches deep, into one ſide of which, near its middle, there was inſerted a pipe one inch diameter; to this pipe was joined another about 3 feet long, paſſing through a trough filled with water, and connected with the hydraulic bellows, the latter being ſlowly elevated, were filled with the air which had paſſed through the burning charcoal in the chaffing diſh, and this air, upon being poured out of a cup over a lighted candle, extinguiſhed it immediately. Large inhalations were made of it by ſome of my aſſiſtants, without injury to themſelves; but, upon me, it produced effects ſimilar to thoſe of fixed air. Its uſes in medicine I cannot pretend to predict; but if azotic air is found uſeful, this may be given in any caſe, wherein fixed air will not be hurtful.

I remain, dear Sir,

Your's,

James Watt.

To Dr. Beddoes.

September 24th, 1794.

ADDITIONAL DIRECTIONS.

To facilitate the ſeparation of fixed or other acid vapours, a wooden ſtirrer or agitator, in the form of an inverted ⊥, with a ſmall winch on the upper end, and a pivot at bottom is put into the refrigeratory, the ſhank going up through the ſhort pipe left for the exit of the hot water; by mixing ſome quick-lime with the water, and uſing this agitator, all acid vapours will be quickly abſorbed.

Sometimes by ſhaking the apparatus, the joints about the fire tube or pot are apt to become untight. This is cured by luting them on the outſide, with a paſte made of freſh ſlaked lime and linſeed oil, but care muſt be taken not to make the joints with this lute originally; for if it penetrates the inſide, it will give a very bad ſmell to the air.

When oxygene air is produced from Mendip manganeſe by heat, there ought to be a ſmall hole ſtopped by a wire, in the conducting pipe near the refrigeratory, on holding a candle near this hole you will ſee by the brightneſs of the flame, when the manganeſe begins to give oxygene air. The ſmall hole is then to be ſtopped, and the air which has previouſly come over is to be diſcharged from the bellows, and alſo by opening this hole from time to time, you will ſee when the oxygene air ceaſes to be produced; and during its production the agitator ſhould be uſed, to ſeparate the fixed air mixed with it.

(This ingenious idea is applicable to any manganeſe. Perhaps a very ſmall tube, ſoldered firm at right angles into the ſide of the conducting pipe would anſwer beſt. T. B.)

If the oxygene is to be ſeparated from nitre by heat, or from manganeſe by means of vitriolic acid, the kneed receiving pipe of the refrigeratory ſhould be of earthen ware or glaſs, and the conducting pipe from the pot of glaſs. The acids have not a ſpeedy action upon the parts of the refrigeratory which are under water, eſpecially if the water is mixed with lime, as it ought to be in ſuch caſes.

Sept.

Sept. 27*th*, 1794.

DEPHLOGISTICATED AIR FROM MANGANESE BY HEAT.

Having obtained a ſmall quantity of Exeter Manganeſe, ſince the preceding papers were printed, I am enabled to give ſome directions concerning the proceſs for extricating the air from it by mere heat.—The manganeſe, reduced to a coarſe powder, is to be put into the pot or the fire tube, which are then to be placed in the furnace; and the joints with the capital or end pieces carefully made with the cement of China clay, or quick lime moiſtened with ſaturated ſolution of borax.—The reſt of the apparatus is then to be put together, and the joints of the conducting pipe with the refrigeratory, and of the refrigeratory with the bellows, to be made good with a part of China clay, or whiting, mixed with flour and water, and for farther ſecurity a rag bound round them.—When this is done, the fire is to be lighted, and no more air admitted than will permit it to burn very gently; for a very moderate heat is ſufficient for diſengaging the air.—When the cement of the joints near the fire is dried, they ought to be covered over with the fat lute of lintſeed oil and ſlaked lime, and when that liquifies by the heat, a little dry lime may be applied to it to prevent its running off.—The hole for the water-pipe may either be ſtopped with a plug, or with the water-pipe itſelf, which in that caſe ſhould be filled with water, merely to inſure its being air tight; for it does not ſeem neceſſary to admit water in this proceſs, though it ſeemed rather to accelerate the extrication, it did not appear to increaſe the quantity of the air.—A pound of the hard part of the Exeter manganeſe yields about 1400 cubic inches of air, which appeared highly dephlogiſticated, by its action on the flame of a candle, and free from any taſte or ſmell of fixed air; (I did not ſubmit it to other teſts; the ſtate of my health does not permit me to make experiments with nitrous air.) The ſoft clay-like part of this manganeſe does not yield ſo much air, but what it yields is equally pure.—I had imagined that the fire-tube might be emptied, and freſh manganeſe put in, without removing the tube from the fire, but I find that

that the manganese parts with its air so easily, that most of the air would be lost before the joints of the tube could be made good again. It is therefore proper to take the tube out of the fire, and let it cool before it is refilled. The fire-tube of the small furnace will hold as much manganese as will yield 2 cubic feet of air, and the large tubes or the pot twice as much.

October 2d.

FURTHER ADDITIONAL DIRECTIONS.

This publication being delayed, on account of the engraver's having failed in having the plates ready, at the time he promised, gives an opportunity of adding some remarks which may be useful.

Chalk gives out its fixed air by heat, more readily than marble does, and if it be previously broke into bits, about $\frac{1}{4}$ inch cube, and soaked in a large quantity of water, which should be once or twice changed, there will be no danger of the purity of the air being degraded by marine acid, or other heterogeneous matters.

Half a pound of dove-coloured marble yielded about $1\frac{1}{2}$ cubic feet of air, of which 2-3ds were absorbed by lime water, and part of the remaining third was inflammable. This air had an extremely bad smell, probably, owing to the colouring matter of the marble.—Statuary marble has not been tried by me in this way.—Neither very great heat nor much water seem necessary in this process.—The marble gave out its air much more slowly than the chalk.

It has been mentioned that two different sizes of this pneumatic apparatus are made; the larger has been recommended for hospitals, apothecaries, and others who want large quantities of the airs, and the smaller for the use of private persons.

On trial it appears, that the smaller furnace, with its fire tubes, answers much better than was expected, and that it may be very well conjoined with the larger bellows and refrigeratory, for those who do not want *very* great quantities of air. The small fire tubes will hold about three pounds of Manganese, which if it be of the Exeter kind will make about two cubic feet of dephlo-

gisticated

giſticated air. Theſe tubes will hold about one pound and a half of chalk, which will yield about four cubic feet of fixed air; they will hold about two pounds of hammered iron turnings, which makes a large quantity of inflammable air, and their contents of charcoal will laſt ſome hours, making a cubic foot of air in 6 or 8 minutes.

It is only in making the air from zinc that they are deficient, that metal ſublimes and ſtops up the end pieces; for this purpoſe the pot ſeems neceſſary; but the pot can be heated in a temporary furnace of looſe bricks, which renders it unneceſſary to have the large furnace on that account.

The fire tubes of the larger furnace being three inches inſide diameter, and having 14 inches in length expoſed to the fire, will hold large quantities of the materials, and will probably make proportional produces of air, but tubes of this ſize have not yet been tried; thoſe of two inches diameter have been tried in this furnace and anſwer very well.

As ſilk bags are coſtly and not very durable, it is thought that the larger bellows may be uſed to breathe the mixed airs out of; for this purpoſe a flexible tube with a mouth-piece ſhould be inſerted into one of the pipes at bottom of the bellows; ſo that the patient may not be confined to one poſture while inſpiring the air. Theſe tubes may either be made of caoutchouc, according to a proceſs deſcribed by Mr. Groſſart in the Annales de Chemie, a tranſlation of which is contained in the three firſt numbers of the Repertory of Arts and Manufactures now publiſhing in London, or they may be made of the new water-proof leather for boots and ſhoes.—The leather ſhould firſt be deprived of its ſmell, by the means directed for the oiled ſilk, then ſowed up neatly upon a mould by a ſhoemaker, and the ſeam varniſhed with japanners gold ſize diluted with oil of turpentine; if the tube be more than half an inch diameter, it ſhould be ſowed together upon a ſpiral of braſs wire, to prevent its cruſhing in bending, or if any noxious qualities be feared from the wire, a thin ſtrip of whalebone may be uſed, or ſtrips of the oſier, of which fine baſkets are made. In any caſe there ſhould be a wooden muold

mould within the wire, until it is ſowed up.—Where the water-proof leather cannot be had, good tanned calf leather may be uſed, and it may be made air tight by anointing it warm, with the following compoſition, taking care to ſoak it well in, by help of the heat.

Take Bees wax one ounce.
White roſin one quarter of an ounce.
Olive oil half a drachm.
Oil of turpentine 3 ounces.

Diſſolve the other ingredients in the oil of turpentine by heat, and always warm it before you uſe it.

Some excuſe ſhould be made to the publick for the deſultory and incorrect manner in which theſe directions are written. The truth is, the ſubject was nearly new to me, my knowledge has advanced in conſequence of experiments, and I have therefore added whatever I thought might be uſeful, as it occurred, and I have been more ſtudious of things than of words. As they are, I hope they will be of ſome uſe, and in that caſe, I ſhall feel amply rewarded for my labour and attention.

James Watt.

N. B. Thin oiled ſilk, though well oiled, I have not found to anſwer ſo well as thick. I have directed an hydraulic bellows of 2 or 3 cubic feet contents, for patients to breathe out of, and think this preferable to oiled ſilk bags. Both will be uſeful. T. B.

CONTENTS.

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REVIEWS.

In the whole art of reviewing books, no rule ſeems more uſeful than the following: *When you would appear wondrous wiſe, ſubſtitute ſome different ſenſe in place of that of the Author; and then proceed, according to your ſect or humour, to inflict criticiſm upon your own miſrepreſentation.* In the CRITICAL REVIEW for September, 1794, p. 9. there occurs a pretty exemplification of this canon. Here it is: "Dr. Beddoes, "in his introductory letter to Dr. Black, obſerves: *You "have heard the project vilified. So would a Panacea be. So "was the Peruvian bark; and inoculation; and every great "improvement of that art, from which, according to its ſtate, all "in their turn ſhall experience good or harm.* This, however, "is a mode of reaſoning which may with equal propriety be "adopted by every perſon who wiſhes to impreſs a novelty "on the public. The public did very wiſely in declining "the uſe of inoculation and Peruvian bark, till their utility "was confirmed by facts." Here, equitable reader! decide whether cenſure is not founded upon ſome ſuppoſed aſſertion abſolutely foreign from the text. "The project," ſays the author, "has been vilified or condemned before trial." You talk unreaſonably, rejoins the critic, *for*, in the uſe of medicines, is it not right to be governed by experience?—This rejoinder is evidently inapplicable; becauſe thoſe quick-witted Doctors who damn a new propoſal off-hand at firſt hearing, are quite a different ſort of people from ſuch as, with a deſire for knowledge, abide the reſult of experience. Among theſe ſlow-minded men, the author ranks himſelf. He is obliged to wait for evidence before he forms any opinion concerning the virtues of elaſtic fluids. He avows, however, ſome ſcorn for that pretended wiſdom, which declines the uſe of a new remedy, where the patient is ſure to die, if nothing beyond the common routine is attempted. He believes it rather probable, that if nothing new is tried, Medicine will for ever remain as helpleſs as at preſent. Had he lived in the middle of the 17th century, he ſhould not have been ſo very wiſe as to decline the uſe of the Peruvian bark in intermittents. Again, what does this reviewer mean by *the Public?* If inoculation and the bark had been generally rejected, whence were facts to come in evidence of their efficacy or inefficacy? If the public is here put for all but a few practitioners, why ſhould the means of acquiring certainty be limited? Is not induction the ſafer, the larger?

The

The following profound ſentence ſeems to involve another equally warrantable aſſumption : "In reviewing two "former works of Dr. Beddoes, publiſhed with a view to "introduce his pneumatic method of curing diſeaſes, WE "remarked that *decided facts* were wanting to recommend it." Now in the firſt of theſe publications, Dr. B. had only offered conjectures ; and did not pretend to adduce facts, having then made no trial. In the 2d having found the project practicable, he announced it as ſuch. He wiſhed to incite men of ſcience and ingenious phyſicians to aſſiſt in this moſt important inveſtigation, confidently hoping, that ſome valuable diſcovery would be made, and theſe hopes have been already realized. At the time of that 2d publication, he had only made one imperfect trial, which was rather favourable ; he deſcribed it as the only one. What ſagacity, therefore. in the critic who could perceive a *paucity* of facts in theſe publications !—It is uſeleſs to notice ſtrictures in Reviews, proceeding from negligence or moderate malice ; but falſe repreſentations deſerve the ſhame of public contradiction.—This perſonal diſcuſſion ſhall cloſe with an hint towards the improvement of literary journals, which, when well conducted, are unqueſtionably uſeful. Were a new Review to be eſtabliſhed, it might be named THE CRITIC AND HYPERCRITIC. The firſt part might be ſimilar to the preſent Reviews, but ſhould exhibit a fuller view of foreign literature. The ſecond ſhould conſiſt of ſuch replies, literally given, as the author or any of his friends ſhould chooſe to make to any criticiſm in any Review, he, the ſaid remonſtrant, paying the whole expence of this part of the publication. The *Jena* Journal, (by far the beſt upon the Continent) is on ſome ſuch plan. The conſequences attending this arrangement would be theſe. 1. The reader would have more matter for his money. 2. Where the author and reviewer were keen dialecticians, they would make good ſport. 3. With this check, books would be always read and commonly underſtood by the Reviewers.—A blank leaf tempted the writer to this retort. He thinks with thoſe who are for diſregarding nonſenſical criticim. The Reader, whom it offends, may cut out, and burn or otherwiſe deſtroy, this leaf, without mutilating the Pamphlet.

E N D.